XXI. *Hiſtory of the Congelation of Quickſilver.* *By* Charles Blagden, *M. D. F. R. S. Phyſician to the Army.*

Read June 5, 1783.

THE late experiments at Hudſon's Bay have determined a point, upon which philoſophers not only were much divided in their opinion, but alſo entertained, in general, very erroneous ſentiments. Though many obvious circumſtances rendered it improbable, that the term of mercurial congelation ſhould be five or ſix hundred degrees below 0 of FAHRENHEIT'S ſcale, as had been at firſt ſuppoſed; yet ſcarcely any one ventured to imagine that it was ſhort of 100°. Mr. HUTCHINS, however, has clearly proved, that even this number is far beyond the truth; and that quickſilver freezes in a degree of cold not exceeding that which ſometimes occurs in the northern parts of Europe, and frequently in the more rigorous climates of Aſia and America. It now appears, that quickſilver, ſo far from containing any eſſential principle of fluidity, does not differ from ſome of the other metals, in its melting point, nearly ſo much as they differ among themſelves; and as it is malleable in its ſolid ſtate, and after calcination recovers its metallic form by heat alone, without the addition of inflammable matter, there can be no doubt but it muſt be ranked among the perfect metals, which, therefore, arranged accord-

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ing to their ſpecific gravity, are theſe four: platina*, gold, quickſilver, and ſilver.

In the general progreſs of ſcience, it is always uſeful at intervals, and eſpecially when any conſiderable advance has been made, to look round and contemplate the proſpect left behind. Thus our actual ſituation is more diſtinctly comprehended, and a better judgement may be formed of what remains to be done. For this reaſon, I thought it might not be unacceptable to the Society, if I were, on the preſent occaſion, to lay before them an account of the different obſervations and experiments I have been able to collect, relative to the congelation of quickſilver; eſpecially as many of theſe are recorded in books not eaſily procured, and in languages little underſtood by the learned of this country. I ſhall begin with the various attempts which have been made to render this metal ſolid by frigorific mixtures, and then enumerate the many inſtances in which that effect has certainly been produced by the natural cold of the air.

PART I.

§ 1. IT was undoubtedly M. JOSEPH ADAM BRAUN, Profeſſor of Philoſophy in the Imperial Academy at Peterſburg, who firſt, upon deciſive evidence, eſtabliſhed the fact, that quickſilver can be made ſolid by a diminution of its heat: but, as in moſt diſcoveries, much depends upon accident, ſo, on this occaſion, M. BRAUN undertook the experiments for a very different object from that which preſented itſelf in the courſe of them, and at the ſuggeſtion of another perſon. This gentleman was Dr. JOHN ERNEST ZEIHER, Profeſſor of Mechanics in the ſame Academy, who having repeated FAHRENHEIT'S experiments with frigorific mixtures in Germany, before he came to ſettle at Peterſburg, wiſhed to try whether they might not be proſecuted further in the great natural cold which ſometimes prevails in that city. Illneſs prevented Dr. ZEIHER from carrying his ideas into execution; he therefore communicated them to Profeſſor BRAUN, who was already much converſant in thermometrical experiments, and engaged him to take up the ſubject of artificial cold whenever the weather ſhould be favourable for this purpoſe. A proper opportunity occurred on the 14th of December, 1759, O. S. the thermometer ſinking in the open air ſo low as $-34°$ of FAHRENHEIT'S ſcale, which we now know to be within a few degrees of the point at which mercury freezes. M. BRAUN accordingly prepared a frigorific mixture with *aqua fortis* and pounded ice, by means of which his thermometer was reduced to $-69°$, lower, by almoſt 30 degrees, than it had fallen in any preceding experiments of this nature.

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* Unleſs, indeed, the irreducible black powder, obtained by M. TILLET, ſhould be thought to place platina among the imperfect metals. See Mem. de l'Ac. Roy. des Scienc. 1779, p. 404, &c.

Part of the quickſilver had now really congealed, yet ſo far was M. BRAUN from entertaining a ſuſpicion of that ſort, and ſo entirely were his views directed to another object, that he acknowledges he had well nigh deſiſted from all further trials, content with having thus much exceeded ſuch eminent philoſophers as FAHRENHEIT, MUSSCHENBROECK, and REAUMUR.

Animated, however, by the hope that a ſtill greater degree of cold might be produced, he entered upon the experiment anew; and all his pounded ice being expended, he was fortunately obliged to ſubſtitute ſnow in its place. With this freſh mixture he had the ſatisfaction of ſeeing the mercury in his thermometer ſink to – 100°, and in ſucceſſive experiments to – 244° and – 352°. Surpriſed at ſo unexpected an event, he drew the inſtrument out of the mixture, and carefully examined its bulb, to ſee if it had received any injury; but he found it perfectly entire, and moreover perceived a much more unexpected phænomenon, that the quickſilver was fixed, and remained immoveable above 12 minutes. On repeating the ſame experiment with another thermometer, graduated no lower than – 220°, all the mercury ſunk into the ball, and became ſolid as before, not beginning to re-aſcend till after a ſtill longer interval of time.

From theſe appearances the profeſſor very juſtly concluded, that the quickſilver in both inſtruments had been fixed or frozen by the cold; but as the evidence was not yet complete, he only ventured to propoſe the congelation of mercury as a *probable truth*, at the next meeting of the Academy held three days afterwards; and in the mean time was making preparations to acquire more palpable proofs of the fact. The thermometers ordered with this view were not ready till the 25th of December O. S. when, in company with the celebrated ÆPINUS, Profeſſor of Phyſics, he performed the experiment with ſimilar materials, and as ſoon as he found the quickſilver immoveable, broke the bulb of his thermometer. Now all his doubts were removed; he obtained a ſolid ſhining metallic maſs, which extended under the ſtrokes of a peſtle, in hardneſs rather inferior to lead, and yielding a dull dead ſound like that metal. Profeſſor ÆPINUS was occupied at the ſame time in ſimilar experiments, employing both thermometers and ſimple tubes of a large bore; with which laſt he remarked, that the quickſilver in them fell ſenſibly on freezing, and aſſumed a concave ſurface; likewiſe, that the congealed pieces would ſink in fluid mercury; all evident proofs of its great contraction. Theſe obſervations were frequently repeated during the winter, with ſome variety in the circumſtances and phænomena, by Profeſſor BRAUN and many other perſons; I find M. LOMONOSOV, Profeſſor of Chemiſtry, the Apothecary-general MODEL, Meſſ. KRASE, HIMSEL, and POISSONNIER, quoted on different occaſions as witneſſes, they having all either performed or aſſiſted at the experiments. Such evidence one would have imagined ſufficient to place the fact beyond all controverſy, and render the congelation of mercury one of the moſt acknowledged truths in natural philoſophy.

It may not here be improper to remark, as an additional proof how much we are indebted to accident for diſcoveries, that if Mr. BRAUN had chanced to begin with a ſpirit thermometer inſtead of a mercurial one, we might very poſſibly to this day have remained ignorant that quickſilver would freeze. For ſince, to judge from Mr. HUTCHINS's experiments, the former would have ſunk but a few degrees in the frigorific mixture, it is not improbable, that the profeſſor, diſcouraged by ſuch a failure of ſucceſs in the immediate object of his purſuit, would have relinquiſhed all further attempts of this nature.

When the ſeaſon for experiments requiring cold was paſt, Profeſſor BRAUN employed himſelf in drawing up a general account of ſuch as he had then made, which he communicated to the Peterſburg Academy on the 6th of September, 1760, O. S. and printed ſoon afterwards as a ſeparate diſſertation*. Of this ſo copious an extract, by Dr. WATSON, is already inſerted in the Philoſophical Tranſactions[*], that it would be improper for me to dwell upon any further particulars. I will only obſerve, that though many of the circumſtances mentioned by M. BRAUN were not firſt remarked by himſelf, yet the diſſertation is compoſed entirely in his name, all the other gentlemen very generouſly giving up their part to him who made the original diſcovery, and undertook to put the whole in a fit dreſs to appear before the world.

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* De admirando frigore artificiali diſſertatio.

Five years afterwards, Profeſſor BRAUN again addreſſed the publick on the ſame ſubject, under the title of "Supplements" to his former diſſertation[†]. Here he declares, that ſince the firſt diſcovery he has ſuffered no winter to elapſe without making ſimilar experiments, and never failed of ſucceſs in freezing the quickſilver, whenever there was a proper degree of natural cold, which he ſtates at − 10°, in order for the experiment to be complete, though ſome commencement of congelation might be perceived when the temperature of the air is as high as + 2°. He confirms all his former obſervations, and adds many others to illuſtrate them; among which two are very important, as coming nearer than any yet known to aſcertain the real contraction that quickſilver ſuffers in becoming ſolid. At the ſame time it muſt be confeſſed, he has not rectified any of his former miſtakes: he retains the ſame groundleſs opinions relative to the freezing point of the quickſilver, the prodigious cold generated by his mixtures, and the explanation of various phenomena, which depend upon very different principles, from thoſe to which he aſſigns them.

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[*] Vol. LII. p. 156.

[†] Supplementa de Congelatione Mercurii. Nov. Comment. Acad. Scient. Imperial. Petropol. tom. XI. p. 302. & ſeqq.

The general ſtate of M. BRAUN's experiments is, that with the above-mentioned frigorific mixtures, and once, when the natural cold was at – 28°, with rectified ſpirits and ſnow, he congealed the quickſilver, and diſcovered moſt of its properties in a ſolid ſtate, eſpecially that it is a real metal, which melts with a very ſmall degree of heat. But not perceiving the neceſſary conſequence of its great contraction in freezing, though aware of the fact, he perpetually confounded the diminution of its volume from this cauſe with that which is ſimply the effect of cold. Hence he conſidered, as the commencement of congelation, what was, in reality, its extreme term, or the utmoſt contraction which the whole would ſuffer in becoming ſolid. To this, indeed, he ſcarcely ever attained, owing to the various impediments that occurred from adheſion of the quickſilver in the thermometrical tube, hollows left in the bulb as it froze, portions of the mercury remaining uncongealed, and many other cauſes. All theſe being by their nature very irregular, his ſuppoſed freezing point came to be extremely uncertain, and ſeveral anomalous appearances were occaſioned, which could not poſſibly be explained upon his miſtaken ſuppoſition; but, notwithſtanding ſuch errors, the greateſt part of our preſent knowledge on the ſubject of mercurial congelation is to be found in the writings of M. BRAUN, who may therefore juſtly be ſtiled the father of this branch of ſcience.

In his ſupplementary treatiſe, the Profeſſor engages to continue his reſearches, and to lay the reſult of them before the Academy, if they ſhould lead to any thing new. But he did not live to accompliſh his deſign. His original diſſertation was re-printed, and the ſupplement firſt publiſhed, in the XIth tome of the Novi Commentarii Academiæ Scientiarum Petropolitanæ for the year 1765. This volume did not appear till 1767, and Profeſſor BRAUN died the following year. Theſe two treatiſes contain the ſubſtance of all his obſervations; but ſeveral further particulars, relative to the diſcovery, may be collected from the Philoſophical Tranſactions *, the Hiſtory of the French Academy of Sciences †, and other literary publications of that period.

§ 2. Profeſſor BRAUN, in his firſt diſſertation, expreſſed a very commendable wiſh, that the experiment of congealing quickſilver might be repeated in other countries, and laments, in the ſupplement, that nothing of this kind had been done. It was not, however, till the year 1774, that his aſſertions received any ſort of confirmation out of Ruſſia, and then by a mode of experiment which did not ſeem to promiſe much ſucceſs. M. JOHN FREDERIC BLUMENBACH, then a ſtudent of Phyſic at Gottingen, now Profeſſor of Medicine in the ſame Univerſity, obſerving the intenſe cold that prevailed there in the month of January that year, took the opportunity of expoſing ſome quickſilver to its action. As the original account of this experiment was given only in a ſingle number of the Literary Journal of Gottingen ‡, in the German language, I will here tranſlate it as exactly as poſſible, ſeveral of the circumſtances being very remarkable.

"On the 11th of January," ſays M. BLUMENBACH, "at "half after five in the evening, I put three drams of quick- "ſilver in a ſmall ſugar-glaſs, and covered it with a mixture of "equal parts of ſnow and Egyptian ſal ammoniac. This mix- "ture was put looſe into the glaſs, ſo that the quickſilver lay

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* Vol. LI. p. 670.

† Hiſt. de l'Académie des Sciences, 1760, p. 26.

‡ Göttingiſche Anzeigen von gelehrten Sachen. Stück 13. Jan. 29, 1774.

"perfectly free, being only covered by it as with pieces of ice; "the whole, together with the glaſs, weighed ſomewhat above "an ounce. I hung it out at a window three ſtories high, "upon a ſmall roof facing the weſt, ſo that the glaſs was "freely expoſed to the north-weſt; and I mixed with the ſnow "upon which it ſtood two drams more of ſal ammoniac. The "ſnow and ſal ammoniac in the glaſs ſoon froze in the open "air to a maſs like ice: no ſenſible change, however, appeared "in the quickſilver that evening; but at one in the morning it "was found frozen ſolid. It had divided into two large and "four ſmaller pieces; of the former, one was hemiſpherical "and the other cylindrical, each ſeemingly rather above a dram "in weight; the four ſmall bits might amount to half a ſcruple. "They were all with their flat ſide frozen hard to the glaſs, and "no where immediately touched by the mixture; their colour "was a dull pale white, with a bluiſh caſt, like zinc, very "different from the natural appearance of quickſilver. I wiſhed "much to break the glaſs immediately, and to try how theſe "bodies would bear the hammer; but deſiring rather to have "witneſſes of ſuch a rare phænomenon I refrained. The ſpirit "of wine, in an excellent thermometer made by BRANDER, "ſtood at this time 10° under 0 of FAHRENHEIT's ſcale, which "was the cold of Upſal in 1740. Next morning, the 12th, "about ſeven o'clock, I found that the larger hemiſphere began "to melt, perhaps becauſe it was moſt expoſed to the air, "and not ſo near as the others to the ſal ammoniac mixture "which lay beneath. In this ſtate it reſembled an amalgam, "ſinking to that ſide on which the glaſs was inclined, but "without quitting the ſurface of the glaſs, to which it was "ſtill firmly congealed; the five other pieces had not yet undergone "any alteration, but remained frozen hard, as I had

" obſerved them in the night. I now made haſte to call ſome " of my friends, who came in time to ſee evidently all this " with me. They were the younger Dr. VOGEL, and Meſſ. " WEBER, WAGNER, and GRAUMANN. Toward eight o'clock " the cylindrical piece began to ſoften in the ſame manner as " the former, and the other four ſoon followed. About eight " they fell from the ſurface of the glaſs, and divided into many " fluid ſhining globules, which were ſoon loſt in the interſtices of the frozen mixture, and re-united in part at the " bottom, being now exactly like common quickſilver."

In this experiment at Gottingen it is not a little ſurpriſing, that the cold ſhould have proved ſufficient to effect ſuch a congelation of the quickſilver. A mercurial thermometer would probably have ſtood lower than M. BLUMENBACH's of ſpirits; but how much cannot poſſibly be determined, without knowing the ſtrength of the ſpirits, and after what method it was graduated. At all events, the difference could ſcarcely have amounted to ten degrees, which would ſtill give the temperature of the air at leaſt 20° above the freezing point of quickſilver. Sal ammoniac with melting ſnow produces 32 degrees of cold; may we ſuppoſe, that it has a power of increaſing the cold nearly as much when it and the ſnow are previouſly cooled below that point *? But if ſo, why was not the quickſilver congealed till after a period of ſeveral hours, ſince other frigorific mixtures begin to act almoſt immediately? Beſides, there was not here the appearance of action, which conſiſts in a ſolution of the ſnow, inſtead of its freezing into a maſs. Or had the cold been greater where the mercury was placed than at M. BLUMENBACH's thermometer? The whole experiment remains

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* See Biſhop WATSON's Chemical Eſſays, vol. III. p. 138.

involved in ſuch obſcurity, that ſome perſons have ſuppoſed the quickſilver itſelf was not frozen, but only covered over with ice, to which opinion, however, there are great objections.

It is worthy of remark, that Gottingen, though ſituated in the ſame latitude as London, and enjoying a temperate climate in general, becomes ſubject at times to a great ſeverity of cold. This of the 11th of January, 1774, is one inſtance. I find others when the thermometer ſunk there to −12°, −16°, or −19°; and at Cattlenburg, a ſmall town about two German miles diſtant, to −30°[*]. By watching ſuch extraordinary occaſions, experiments on the freezing of quickſilver might eaſily be performed in many places where the poſſibility of them is at preſent little ſuſpected. The cold obſerved at Glaſgow in 1780 would have been fully ſufficient for that purpoſe[†].

Dr. BLUMENBACH's deſcription of the ſolid quickſilver differs ſo much from Profeſſor BRAUN's, with reſpect to its colour and general appearance, as to require a particular explanation. Their diſagreement, I imagine, was occaſioned by a diverſity in the circumſtances of their experiments. Quickſilver cryſtallizes in becoming ſolid. In this property it reſembles other metallic ſubſtances, as appears from many facts, and is elegantly exemplified in thoſe curious cups which are formed by expoſing proper maſſes of melted metal to the cold air till the outer part be ſufficiently hardened to conſtitute a ſolid coat, and then letting out the internal fluid part, ſo as to leave a hollow in the middle. This concavity is found every where beſet with metallic cryſtals, ſcarcely yielding in beauty and regularity to the fineſt configurations of ſalts. In like manner, with regard to quickſilver, Profeſſor BRAUN himſelf obſerved, that whenever it had congealed but imperfectly, and the fluid part was poured off, the ſolid ſurface which came in view was extremely rough, as if compoſed of many ſmall globules. One of Mr. HUTCHINS's late obſervations exceedingly illuſtrates this matter; for he remarks, that when the fluid mercury was decanted off, in his tenth experiment, "the internal ſurface of the frozen "quickſilver ſhewed very uneven, with many radii going acroſs, "ſome of which had heads reſembling pins." Now in Profeſſor BLUMENBACH's experiment, the quickſilver lying looſe, except the flat ſide that touched the glaſs, could cryſtallize without impediment, and hence aſſumed a rough, and conſequently a dead-white ſurface; whereas in thoſe made by Mr. BRAUN, with tubes and thermometers, the metal being ſo much confined by the ſmooth glaſs, its ſurface was rendered of a high poliſh, not diſtinguiſhable in point of ſplendor from that of fluid mercury. Perhaps alſo, M. BLUMENBACH's quickſilver might have been made to look duller by ſome dirt or moiſture collected upon it from the ſal ammoniac and ſnow.

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[*] LAXMANN's Sibiriſche Briefe, p. 98. Nov. Commentar. Petrop. tom. VII. p. 396.

[†] Viz. −23° on the ſnow, −14° in the air. Phil. Tranſ. vol. LXX. p. 456.

§ 3. In the notification of Profeſſor BRAUN's experiments by the French Academy of Sciences, a ſort of requeſt is inſerted, that proper perſons might be ſent by the Ruſſians into Siberia, and by the Engliſh to Hudſon's Bay, for the purpoſe of repeating them with every advantage of natural cold. I do not find that the Ruſſians have taken any notice of this, and indeed with regard to them it would be almoſt ſuperfluous, as ſcarcely a winter paſſes at Peterſburg in which the weather is not ſevere enough for the ready congelation of mercury by artificial means. But in this country, the Royal Society, ever attentive to the improvement of ſcience, deſired their late ſecretary Dr. MATY to make the neceſſary application to the Hudſon's Bay Company; in conſequence of which Mr. HUTCHINS, who was then in London, and going out with a commiſſion as governor at Albany Fort, offered to undertake the experiments, and executed them in a very complete manner, as appears from the account publiſhed in the LXVIth volume of the Philoſophical Tranſactions. In the months of January and February 1775, he twice froze quickſilver at Albany Fort; and in the firſt of theſe experiments, having broken his thermometer, he found, that the metal flattened by a fall of about ſix inches, bore to be hammered, gave a "deadiſh" ſound like lead, and was finely poliſhed on the ſurface. As Mr. HUTCHINS adopted exactly the method of Profeſſor BRAUN, he obſerved the ſame phænomena, encountered the ſame difficulties from the ſticking of the quickſilver in the tube, and cracking of his thermometer, and was equally at a loſs with regard to the point of congelation. Still, however, this was the fulleſt confirmation that M. BRAUN's Diſſertations had ever yet received; and it may be conſidered as a prelude, by which Mr. HUTCHINS acquired the experience that enabled him to ſucceed ſo perfectly in his laſt moſt deciſive and ſatisfactory experiments.

§ 4. The account of Mr. HUTCHINS's ſucceſs at Hudſon's Bay was read before the Royal Society at the commencement of the ſevereſt winter that had been known for many years in Europe. Two gentlemen of different countries embraced this opportunity to attempt the congelation of quickſilver. The firſt was Dr. LAMBERT BICKER, Secretary to the Batavian Society at Rotterdam, who, on the 28th of January, 1776, at eight in the morning, made an experiment to try how low he could reduce the thermometer by artificial cold, the temperature of the air being then +2°. He could not bring the mercury lower than −94°, at which point it ſtood immoveable; and upon breaking the bulb he ſaw with certainty that the outer part of the quickſilver had loſt its fluidity, and was thickened to the conſiſtence of an amalgam; it fell out of the bulb in little bits, which bore to be flattened by preſſure, without running into globules like the inner fluid part.

Next day, when the thermometer ſtood at +8°, he repeated the experiment with all poſſible exactneſs, after M. BRAUN's manner; but could not obtain a greater deſcent of the mercury than to 80° under 0, and did not again break his thermometer.

The firſt account I ſaw of theſe experiments was in an extract from the Altona Poſt[*], in the German language, which Sir JOSEPH BANKS had the goodneſs to procure from Gottingen at my requeſt; but as they are confirmed by Profeſſor VAN SWINDEN in his Obſervations on the cold of 1776[†], there can can be no doubt of their authenticy; and they afford a proof that the freezing point of quickſilver cannot be lower than −94°, as it ſunk only to that degree, and yet was in part congealed.

§ 5. The other gentleman who tried the effect of this ſevere cold in 1776 upon mercury was Dr. ANTHONY FOTHERGILL, at Northampton, and the account of his experiment may be ſeen in the Philoſophical Tranſactions for that year[||]. His frigorific mixture appears to have been made with the vitriolic acid; and the natural cold of the air at Northampton that day, the 30th of January, was +9°. It is ſcarcely poſſible to determine how far he ſucceeded. The quickſilver of his thermometer ſunk into the bulb, and it, as well as ſome in a phial, contracted what Dr. FOTHERGILL calls a film on the top; but unleſs the ſcale of his inſtrument went below -40°, or ſome ſolid cryſtals were formed, ſuch as M. BRAUN and others obſerved at the commencement of congelation, nothing can be collected with certainty from this experiment.

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[*] Altonaër Reichspoſtreuter, 1776. N° 24. Feb. 24.

[†] Obſervations ſur le froid rigoureux du Mois de Janvier, 1776, p. 277.

[||] Vol. LXVI. p. 589.

Though the cold of this hard winter was not ſufficient, either in England or Holland, for the convenient performance of experiments on the congelation of quickſilver, yet in many parts of the European continent, not farther north, it was fully intenſe enough for that purpoſe. The very morning when Dr. BICKER ſucceeded ſo imperfectly at Rotterdam, the thermometer ſunk to -22° at Rudolſtadt, ſituated a degree more to the ſouthward; and it had been the preceding day as low as -18° at Berlin *.

§ 6. I find no further attempts to freeze quickſilver till the year 1781, when Mr. HUTCHINS reſumed this ſubject with ſuch brilliant ſucceſs. The preceding experiments had done little more than prove that quickſilver might be rendered ſolid by cold, and ſhew what ſort of ſubſtance it was in that ſtate. Nothing ſatisfactory had been aſcertained with regard to its freezing point, or the degree of a thermometer at which it ceaſes to be a melted and becomes a ſolid metal. It muſt not be ſuppoſed, however, that the gentlemen who were engaged

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* Beſchäftigungen der Berliniſchen Geſellſchaft Naturforſch. Freunde. B. II. p. 575, 576.

in theſe reſearches neglected ſuch a principal object of enquiry; on the contrary, Profeſſor BRAUN himſelf, as hath been already mentioned, took infinite pains to inveſtigate it, but, for want of perceiving the conſequences of the metal's great contraction in becoming ſolid, went very wide of the truth. This ſource of error did not eſcape the penetration of other philoſophers, ſeveral of whom declared their opinion that the degree of cold neceſſary for the congelation of quickſilver could hardly be determined by freezing a thermometer filled with that fluid. But Mr. CAVENDISH and Dr. BLACK were the gentlemen who ſuggeſted an adequate method of obviating the difficulty, ſo as to aſcertain the point in queſtion with certainty and preciſion. Reaſoning on the well-known fact, that a quantity of water continues at the ſame temperature from the moment it begins to freeze till the whole is become ſolid, they very juſtly concluded that the ſame would hold good with regard to quickſilver; and Mr. CAVENDISH confirmed this inference by experiments with metals of eaſy fuſion, in which he found a thermometer keep at the ſame degree all the time they were paſſing from a fluid to a ſolid ſtate. Hence it was propoſed, that a ſmall thermometer ſhould be placed in ſome quickſilver to be frozen; which ſinking pretty regularly till the congelation began, and remaining ſtationary till it ſhould be complete, would thus ſhew the degree of cold at which this effect takes place.

It is not at all ſurpriſing, that both the above-mentioned gentlemen ſhould have ſuggeſted the ſame method, without the leaſt communication of each other's ſentiments. Moſt diſcoveries follow ſo cloſely upon a certain ſtate of advancement in every ſcience, that, under the preſent general diffuſion of knowledge, ſimilar ideas muſt be expected to occur about the ſame time to ſuch perſons as are engaged in ſimilar purſuits. When the fruit is come to a certain degree of ripeneſs, more than one man may have ſtrength enough to ſhake it off. Were we ſure that philoſophy would continue to be regularly cultivated, perhaps it might with truth be affirmed, that the utmoſt efforts of genius amount only to a power of anticipating diſcoveries which would neceſſarily be made in the courſe of a few years by the common progreſs of mankind. The principles of this experiment for determining the point of congelation in mercury being already before the world, it was moſt probable, that the conſequences to be deduced from them would not eſcape gentlemen of ſuch acknowledged ſagacity, whenever they might happen to apply their attention to that ſubject.

Though the methods propoſed by Mr. CAVENDISH and Dr. BLACK were fundamentally the ſame, yet there was ſome difference in the apparatus they recommended; and as the former gentleman got *his* executed in London and ſent out to Hudſon's Bay, it was that which Mr. HUTCHINS employed in performing moſt of his experiments. A circumſtantial detail of theſe has ſo recently been read before the Society, that it would be tedious for me here to recapitulate the particulars. They have not only confirmed the preceding obſervations relative to the ſolid ſtate into which quickſilver can be brought by cold, its metalline ſplendor and poliſh when ſmooth, its roughneſs and cryſtallization where the ſurface was unconfined, its malleabillity, ſoftneſs, and dull ſound when ſtruck; but have alſo clearly demonſtrated, that its point of congelation is no lower than −40°, or rather −39°, of FAHRENHEIT's ſcale; that it will bear, however, to be cooled a few degrees below that point, to which it jumps up again on beginning to congeal; and that its rapid deſcent in a thermometer through many hundreds of degrees, when it has once paſt the above-mentioned limits, proceeds merely from its great contraction in the act of freezing. Theſe and the other conſequences deducible from Mr. HUTCHINS's experiments have been ſo exactly pointed out by Mr. CAVENDISH, the real author and firſt mover of the whole buſineſs, that nothing remains for me but to add a few ſupplementary remarks.

Moſt of the appearances which perplexed M. BRAUN in his experiments admit of ſuch a ready ſolution from theſe of Mr. HUTCHINS, that it would be ſuperfluous to dwell upon each particular; there is one, however, which muſt be mentioned, becauſe an erroneous explanation has been given of it by a very eminent Swediſh philoſopher*. Profeſſor BRAUN obſerves, that by a certain kind of management he could effect the congelation of quickſilver with very weak aqua fortis. For this purpoſe he filled ſeveral different glaſſes with ſnow, into which he ſucceſſively poured the dilute acid of nitre, and immerſed his thermometer. The mercury, which would ſink only to − 148° in the firſt glaſs, came in the fourth to the term of complete congelation. It is no wonder that the profeſſor, with the ideas he entertained, ſhould think this extraordinary; but we now clearly underſtand, that the cold in the firſt glaſs was ſufficient to freeze part of the quickſilver, but did not laſt long enough to render the *whole* ſolid; in the ſecond glaſs a further part froze; in the third ſtill more; till at length only ſuch a quantity was left as the fourth mixture could fully congeal.

Another phænomenon, of which M. BRAUN gives a very unſatisfactory ſolution, is the ſinking of the quickſilver in his thermometers after they were taken out of the frigorific mixture. This, I ſuppoſe, proceeded entirely from its beginning to melt in the warmer air, and conſequently ſubſiding to fill up vacuities in the ſtem or ball, in the ſame manner as happened very remarkably in ſome of the experiments at Hudſon's Bay, and in various meteorological obſervations to be deſcribed hereafter.

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* Kongl. Vetensk. Acad. Handlingar vol. XXXIII. p. 119.

Mr. HUTCHINS mentions, in the remarks upon his ninth experiment, that the internal ſurface of the congealed quickſilver, after the ſuperincumbent frigorific mixture and fluid metal had been decanted off, "was every where very rough, and of a "dull white, reſembling that of a ſilver ſpoon in common "uſe;" likewiſe, that "the lump ſhewed very weak cohesion, "crumbling to pieces under the ſtrokes of a hammer, and had "not the uſual poliſh." Theſe differences he aſcribes to an effect produced, by the ſpirit of nitre in the mixture, upon the quickſilver, with which it came into immediate contact; but I am rather of opinion, that they were occaſioned chiefly by the unconfined cryſtallization of the mercury in the open gallipot employed for this experiment, ſimilar appearances having always been obſerved when it congealed under ſuch circumſtances.

It is here neceſſary to take notice, that the thermometers ſent out to Mr. HUTCHINS, even thoſe made with quickſilver, did not exactly agree together. From his *Table of Compariſon* we may collect, what ſubſequent experiments have confirmed, that −40° upon the two ſmall ones with ivory ſcales, anſwered to −44° or −45° upon one of the large thermometers, that which had the ball on top, and to −42° or −43° upon the other. Now as this laſt was the inſtrument which Mr. HUTCHINS plunged into the quickſilver he attempted to freeze in his eighth experiment, and which ſunk only to −40°, it is no wonder that he did not then perceive any marks of congelation, the cold really wanting two or three degrees of being ſufficient for that effect.

The old ſpirit-thermometer called *H* was erroneous no leſs than 7° or 8° near the freezing point of water, which ſerved as a kind of compenſation in the lower parts of the ſcale, ſo as to make it agree tolerably well with thoſe of mercury in the greateſt cold of the frigorific mixtures.

Although in two of Mr. HUTCHINS's thermometers the quickſilver ſunk exceedingly low, to −450° or near −500°, there is reaſon to believe he did not in any inſtance obtain the extreme term of contraction, ſince Profeſſor BRAUN, in ſome of his laſt experiments, brought the mercury in one thermometer to −544°, and in another to −556°[*]. Hence it would ſeem, that Mr. HUTCHINS had always ſome part of the quickſilver left unfrozen, or ſome vacuity remaining, either in the ſtem or the ball of his inſtruments; and as no objection appears againſt thoſe experiments of M. BRAUN's, we muſt conclude, that quickſilver, in becoming ſolid, contracts about a 23d of its whole bulk. When the principal object in view is to determine the quantity of this contraction, it will be moſt expedient to perform the experiment in the leaſt degree of cold which will permit the quickſilver to be entirely frozen, that it may not be ſo likely to ſtick faſt in the tube; but care muſt be taken to congeal the whole of the mercury in the ſtem as well as that in the bulb.

Among the numerous improvements in natural knowledge which have been made within a ſhort period of years, perhaps none tends to illuſtrate more phænomena of nature than the late diſcovery, that a conſiderable quantity of heat diſappears when bodies paſs into a ſtate of fluidity or elaſtic vapour, and re-appears when they are converted back again to their original condition. This remarkable effect of ſuch changes, I believe, was firſt obſerved at Glaſgow, about twenty years ago, by Dr. BLACK and Mr. IRWIN, who endeavoured to determine its moſt material circumſtances by various experiments. Since that time Dr. BLACK has conſtantly taught it in his chemical lectures; and conſidering the heat which diſappears as ſtill remaining in the fluid or vapour, but deprived for the time of its property of being communicated to other bodies, and thereby becoming ſenſible, he calls it *latent* heat, a term ſufficiently expreſſive of his manner of conceiving the fact.

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[*] Nov. Comment. Petrop. tom. XI. p. 313.

In the year 1772, the celebrated Profeſſor WILCKE inſerted, in the Tranſactions of the Royal Academy of Sciences at Stockholm *, a paper profeſſedly on the ſubject of *the cold produced by ſnow in melting*, which being written in the Swediſh language is leſs known in this country than it deſerves. He ſeems not at all acquainted with what Dr. BLACK had done, but ſpeaks of it as his own diſcovery, originating in an accidental attempt to melt away a quantity of ſnow by the affuſion of hot water; when he found the proceſs go on ſo ſlowly, and ſo little effect produced, that he determined to inveſtigate the cauſe of ſo unexpected an event. After a ſeries of experiments with this view, he came to the following concluſion; that ſnow, in melting, conſtantly abſorbs a certain and equal quantity of heat, which is employed entirely in giving it fluidity. To render ſuch a property more intelligible, M. WILCKE propounds a particular theory of an elaſtic fluid between the particles of bodies; and he proceeds to various illuſtrations and deductions, all highly ingenious.

Two principal methods have been adopted to prove this loſs of heat; one, by adding ice at the freezing point to a certain proportion of water at a known degree of heat, and obſerving how much the temperature of the mixture comes out below that which ſhould have reſulted according to the common laws of the diſtribution of heat among bodies; the other, by obſerving how much faſter water near the freezing point acquires ſenſible heat, than an equal quantity of ice melting under ſimilar circumſtances. It is obvious, that both theſe methods tend not only to prove the fact, but likewiſe to diſcover the quantity of heat ſo abſorbed; and that the latter alſo, if the operation be reverſed, will ſhew the quantity of heat evolved, when a fluid congeals or becomes ſolid. In this way Dr. BLACK eſtimates the heat in queſtion to be equal to 140 degrees upon FAHRENHEIT's ſcale; Profeſſor WILCKE, by a great variety of experiments with different proportions of ſnow and water, brought it out pretty uniformly about 130; and Mr. CAVENDISH, as he hath lately informed us, finds it amount to 150, and chooſes to call the proceſs a generation of heat or production of cold *.

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* Kongl. Vetenſkaps Acad. Handlingar vol. XXXIII. p. 97.

As the method put in practice by Mr. HUTCHINS to ſettle the freezing point of quickſilver depends entirely upon this generation, re-appearance or evolution of heat, by means of which the congealing quickſilver is kept at the ſame temperature as long as any conſiderable portion of it remains fluid, I thought ſome account of ſuch an intereſting diſcovery would not here be miſplaced. It now becomes an important object of attention, in examining the properties of bodies, to inveſtigate the quantity of heat produced or loſt at their melting and vaporific points. So little, however, has hitherto been done in this reſpect, even with thoſe bodies that are changing from fluid to ſolid every day before our eyes, that it is no wonder we are yet unable to determine it in a ſubſtance which has ſo ſeldom been ſeen in a ſolid ſtate. But from the very quick congelation of quickſilver when placed in a frigorific mixture, as ſhewn by its

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* Meſſ. LAVOISIER and DE PLACE, from ſome late experiments with their new apparatus, fix it at 135.

rapid deſcent in the thermometer, and from its readineſs to melt again upon an abatement of the cold, apparent in all the experiments, and particularly noticed by M. BRAUN, there is reaſon to believe, that the quantity of heat employed in giving it fluidity is not very conſiderable. When water, which has been cooled below +32°, begins to freeze, a certain part of it, proportioned to the degree of cooling, ſhoots at once into ice; that is, ice continues to be formed till ſo much heat be evolved as is requiſite to bring the whole up again to +32°. Now I am inclined to ſuſpect, that in ſeveral of Mr. HUTCHINS's experiments the firſt jump of the quickſilver down from a little below the point of mercurial congelation, depended on a ſimilar principle of the ſudden freezing of ſuch a proportion of the mercury as correſponded to the number of degrees it had been cooled below that point; hence, if the degree to which it bore to be cooled before it began to congeal, and the contraction it ſuffers in congealing, were both known, its quantity of latent heat, to ſpeak in Dr. BLACK's language, might readily be found. From a rude and vague computation of this ſort, I am led to believe, it is not half that of water; and if ſo, quickſilver ſeems to differ much in this reſpect from other metals; for tin is ſaid, from Mr. IRWIN's experiments, to require, in order to melt, a quantity of heat which, if ſet looſe and rendered ſenſible, would raiſe the thermometer 500 degrees.

Beſides the inſtruments contrived particularly to try the freezing point of quickſilver, two ſpirit-thermometers alſo were ſent out to Hudſon's Bay, principally with a view to ſome collateral circumſtances of the experiment. My intention in recommending them was to diſcover what degree of cold the freezing mixture produced; and to obtain a more exact compariſon of the relative contractions of mercury and alcohol, by marking their ſimultaneous deſcents on a more extended ſcale, or as long as both of them ſhould continue to contract regularly. The ſpecific gravity of the alcohol employed to make theſe thermometers was found to be 0,8254, in a temperature of 58°½; and they were graduated on the principle of two fixed points, one, the real freezing point of water fixed by actual experiment; the other, the 122d degree above 0, determined by compariſon with a ſtandard mercurial thermometer. This interval being divided into 90 parts, the degrees ſo found were meaſured downward as well as upward on the ſcale, with a proper allowance for inequality in the bore of the tube. A ſubſequent experiment ſhewed that the freezing point had been rightly marked upon theſe inſtruments; but that, in conſequence of a common fault in conſtructing thermometers, of not heating the contents of the tube ſo much as thoſe of the ball, the point of 122° was marked on both of them lower than it ought by the ſpace of two degrees; ſo that 122° on the ſcale indicated only 120 degrees above 0 of real heat. Any detail of the obſervations that were made to ſettle the relative contractions of quickſilver and ſpirits by means of theſe inſtruments would be improper at preſent; it is ſufficient to mention, that on one of them the 29th degree below 0, and on the other the 30th, were found to correſpond with − 40° of the ſmall mercurial thermometers, or more preciſely with the point that would have been − 39° upon an exact ſtandard inſtrument.

The other object for which the ſpirit-thermometers were propoſed, is more immediately connected with the congelation of mercury. All former experiments with frigorific mixtures had left us abſolutely in the dark with regard to the degree of cold that was really produced. By theſe inſtruments it is now determined, that the greateſt effect of a mixture of ſnow and ſmoaking acid of nitre, even with the advantage of ſuch natural cold as congealed the quickſilver expoſed to it, was only to diminiſh the heat to ſuch a degree as would correſpond with – 45° or – 46° of a ſtandard mercurial thermometer; and conſequently that the cold obtained by FAHRENHEIT in the firſt experiments with ſuch mixtures, which BOERHAAVE ſtates at – 40°*, cannot be exceeded but by a very few degrees. This reſult is the more ſurpriſing, on account of BRAUN's poſitive aſſertions, that his thermometer both of rectified ſpirits and eſſential oils deſcended 150 or at leaſt 100 degrees below 0 †. But, ſince that gentleman was ſtrongly impreſſed with an opinion of the exceſſive cold neceſſary to freeze quickſilver, ſo much as to ſhew evident perplexity at finding his ſpirit-thermometers ſink leſs than thoſe of mercury, I ſhould, from this circumſtance alone, be inclined to place moſt confidence in the experiments made at Hudſon's Bay, in which no hypotheſis was adopted, and therefore no prejudice can be apprehended. Indeed Mr. HUTCHINS's obſervations with regard to the degree of cold generated by his freezing mixtures, are ſo regular, uniform, and numerous, as hardly to leave a doubt that it does not exceed – 35° or – 36° of his ſpirit-thermometers. And this is, of itſelf, a very great additional proof, that the freezing point of quickſilver cannot be much lower than Mr. HUTCHINS determines it, ſince the mixture was incapable of diminiſhing the heat more than ſix or ſeven degrees further. The advantages ariſing from a knowledge of the cold produced, were ſo apparent in theſe experiments at Hudſon's Bay, with reſpect to many circumſtances, both in the congelation of the quickſilver itſelf,

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* BOERHAAV. Element. Chemiæ, tom. I. p. 164.

† Nov. Comment. Petrop. tom. XI. p. 290. 316. 317. From 260° to 300° of DE L' ISLE's ſcale.

and in the action of the frigorific mixtures, that I ſhould ſuppoſe ſpirit-thermometers will always be employed in future, whenever any thing of this kind is attempted.

§ 7. It is not a little extraordinary, that ſince the death of Profeſſor BRAUN, now near fifteen years ago, all attention to the congelation of mercury ſhould in a manner be laid aſide on the ſpot where it was originally diſcovered. A dead ſilence on the ſubject ſeems to have prevailed at Peterſburgh till this preſent winter; when Dr. MAT. GUTHRIE, F. R. S. Phyſician to the Cadet Corps of Nobles, having heard the matter much canvaſſed during his late viſit here, reſumed the conſideration of it on his return to that metropolis. The only intelligence I have yet received of Dr. GUTHRIE's experiments or concluſions, is contained in one of his letters to Dr. GARTHSHORE of this Society, who has obligingly favoured me with the following extract.

" Having found," ſays Dr. GUTHRIE, " in my late journey " to Britain, that it remained a matter of doubt, whether mer" cury in its pure ſtate, unmixed with heterogeneous matter, " had ever been or was capable of being congealed; I am glad " to be able, from the reſult of ſeveral experiments, to inform " you, that the pureſt mercury known to the chemiſts is capa" ble of congelation, and in that ſtate will bear the hammer.

" I have done ſomething alſo toward determining the point of " its congelation, by determining what it is not, *viz.* 150° of " REAUMUR's thermometer.

" A fine thermometer, made by NAIRNE, graduated 150° " of REAUMUR, that is, 337° of FAHRENHEIT below the " freezing point" [or − 305°] " ſunk entirely into the bulb, " while the mercury in which it was plunged remained per" fectly liquid, nay had not as yet grown thick and gritty, a

" phenomenon that always precedes congelation, as I have " found in my experiments; nor had there as yet been formed " in the inſide of the tube containing the mercury to be frozen " (and the thermometer to determine the point of congelation " with which I ſtirred it) an incruſtation of the metal, another " indication of approaching congelation, which ever begins on " the ſide of the tube, and gradually increaſes till it has " reached the center, and a ſolid cylinder is produced.

" From this you may form a judgement of the impurity of " the mercury which ſome pretend to have ſeen congealed with " natural cold; for here 150° of REAUMUR was not found a ſufficient degree of cold to freeze it, and ſurely no ſuch abſence " of heat, or any thing near it, has ever been, or ever could " be, obſerved on the face of the habitable globe.

" I ſhall only add, that my experiments were conducted on " the plan of my learned friend Dr. BLACK, and ſpiritus nitri " fumans Glauberi with ſnow, were employed to produce an " artificial cold, while the thermometer of REAUMUR ſtood at " 20° below 0 in the open air" [that is, − 13° of FAHRENHEIT's ſcale.]

Though the conſequences here deduced by Dr. GUTHRIE from his experiment are undoubtedly erroneous, as appears from a ſufficient number of other facts, yet it is not at all ſurprizing that they ſhould have ſeemed to him juſt; for the error ariſes from a circumſtance, which could not be foreſeen with certainty, and occurred in ſeveral of Mr. HUTCHINS's experiments as well as in Dr. GUTHRIE's. To underſtand this, it muſt be conſidered, that when we attempt to aſcertain the freezing point of water, by keeping a thermometer immerſed in it while it is changing into ice, the inſtrument employed for this purpoſe is not made of water, but of a different fluid, not ſubject to be peculiarly affected by that particular degree of cold. In order, therefore, to render the experiment with quickſilver perfectly analogous, it would be neceſſary not to make uſe of a mercurial thermometer; but to ſubſtitute ſuch a one as is capable of ſuſtaining a greater intenſity of cold. For otherwiſe, if it ſhould happen, from any circumſtance, that the quickſilver in the thermometer ſhould begin to freeze before that in which it is plunged, the whole experiment muſt evidently be fruitleſs, as the former would ſink, perhaps, many hundreds of degrees in the inſtrument, by its own contraction in becoming ſolid, while the ſurrounding mercury ſtill retained its fluidity. Now this was preciſely the caſe in Dr. GUTHRIE's experiment; the thermometer, with which he ſtirred his quickſilver, congealed, it would ſeem from the great deſcent, almoſt entirely, though he could not perceive in the quickſilver ſo agitated the leaſt appearance of change to a ſolid ſtate. Thus, likewiſe, in ſeveral of the experiments at Hudſon's Bay, the mercury in the encloſed thermometer was found to freeze before that in the cylinder. Hence it is manifeſt, that the continuance of fluidity in a quantity of quickſilver does not ſecure a thermometer of that metal immerſed in it from freezing.

The cauſe of this phenomenon is extremely uncertain. Poſſibly the point of congelation may not be exactly the ſame in all quickſilver under all circumſtances. Foreign admixtures may occaſion a difference in this reſpect; and it does not follow, that the effect of ſuch, in certain proportions, muſt neceſſarily be to make the mercury congeal ſooner, ſince, in the caſe of the fuſible metal, the melting point of tin is brought lower by the addition of two metallic ſubſtances, both of which ſeparately require a ſtronger heat than it for their fuſion.

But as quickſilver bears to be cooled ſome degrees below its freezing point, before it begins to form ſolid cryſtals, the phenomenon in queſtion may depend upon that circumſtance: for if, from whatever cauſe, the mercury in the thermometer ſhould begin to congeal as ſoon as it was cooled down to −39° or −40°, whilſt that which ſurrounded it would ſuſtain a cold of −43° or −44° without becoming ſolid; it is evident, that the whole of the former might be congealed, and yet no part of the latter, though the real freezing point of both were the ſame, that is, though the ſurrounding quickſilver as ſoon as it came to ſhoot its cryſtals would riſe immediately to −39°, the point at which that in the thermometer froze.

As this is undoubtedly the moſt obſcure part of our knowledge relative to the congelation of quickſilver, I endeavoured to illuſtrate it by ſome experiments on the freezing of water. The pureſt water I could obtain bore to be cooled to +21°, no leſs than eleven degrees below the temperature to which it inſtantly roſe as ſoon as the cryſtals of ice ſhot through it. This was diſtilled water very recently boiled; it is a miſtake, therefore, that boiling neceſſarily renders water not ſo capable of being cooled below the freezing point. In proportion as the water was leſs pure, it ſeemed to congeal the ſooner; and the kind of impurity which had the moſt effect appeared rather to be extraneous matter diffuſed through the water, ſo as to trouble its tranſparency, than ſuch as was chemically diſſolved in it *. The ſmalleſt particle of ice, alſo, whenever the water was below the freezing point, either added from without, or by any means formed in it, would inſtantly cauſe a cryſtallization, by which the whole came immediately up to +32°. Likewiſe a crack in the bottom of the containing glaſs veſſel

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* I take this to be the reaſon that boiling has been thought to render water incapable of being cooled below the freezing point. In moſt kinds of water, the application of heat occaſions the precipitation of earthy ſubſtances which were before held in ſolution; hence the water comes to be in the ſtate of having extraneous matter diffuſed through it, and therefore readily congeals.

effectually prevented the water from being cooled below the freezing point, as ice conſtantly formed on the bottom, perhaps in conſequence of the early generation of ſome minute portions of it in the crack. But independently of theſe circumſtances, neither ſtirring, agitation, a current of freſh air on the ſurface, nor the contact of any extraneous body not colder, would cauſe the water to ſhoot into ice, even after it was cooled many degrees below the freezing point, notwithſtanding the repeated aſſertions of authors to the contrary.

How far theſe facts may be applicable to the above mentioned inſtances, where the thermometer froze before the quickſilver in which it was immerſed, can ſcarcely be determined unleſs more particulars were known. They ſhew, however, that the congelation would not neceſſarily be brought on by ſtirring the quickſilver, as practiſed by Dr. GUTHRIE; and point the way to various conjectures upon this difficult phenomenon, the diſcuſſion of which muſt be reſerved for a future opportunity.

This ſource of error in the method for ſettling the point of mercurial congelation, may eaſily be obviated by a ſmall change in the apparatus. Nothing further is neceſſary than to employ thermometers made of alcohol, eſſential oils, or ſuch other fluids as will bear the requiſite cold without freezing. Probably the former of theſe will be found moſt convenient: and although the contraction of other fluids does not exactly keep pace with that of quickſilver, yet as the relative proportions can be readily determined, experiments with them may at all times be reduced to the mercurial ſtandard, being not only the moſt familiar, but likewiſe that which ſeems to correſpond beſt with equal increments and decrements of heat.

There is one way alſo in which mercurial thermometers may be employed to aſcertain the freezing point of quickſilver; I mean by plunging them into ſome of that metal which has been frozen and is now melting. This was put in practice very ſucceſsfully by Mr. HUTCHINS in his ninth and tenth experiments. It anſwers to the method of determining the common point of congelation upon a thermometer by melting ice, well known to be more ſteady and certain in its temperature than freezing water. If, however, the point of mercurial congelation be not exactly the ſame in different portions of the metal, it is evident, that no reliance could be placed on ſuch an experiment; and it can ſcarcely be executed but with the greateſt advantage of natural cold.

As Dr. GUTHRIE was miſtaken in ſuppoſing he had proved that quickſilver did not congeal till it was cooled under −305°, his ſuggeſtion of impurity in the mercury employed by others is clearly without foundation. The inſtances to which he refers, when that metal froze by the natural cold of the air, are rendered certain and unexceptionable, from a great variety of concomitant circumſtances, confirmed by the moſt credible teſtimony, as ſhall preſently be ſhewn*.

§ 8. This account of mercurial congelation by artificial means would remain incomplete, were I not to mention that at Hampſtead, on the 26th of February laſt, the temperature of the air being then above +20°, Mr. CAVENDISH, by an ingenious artifice of diluting the nitrous acid to a proper degree,

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* Since this was written, Dr. GUTHRIE has ſent a more perfect account of his experiments. They agree in the main with Mr. HUTCHINS's, and the difficulties which occurred to him may be ſolved on the ſame principles. It ſeems not improbable, that the thermometer with which the Doctor ſtirred his quickſilver had, by ſome accident, in the courſe of a long experiment, come into contact with the frigorific mixture, and ſo been ſet freezing.

ſunk

ſunk the quickſilver in his thermometer to 110°, and conſequently froze it in part. He then interrupted the experiment to try the cold of his frigorific mixture by a ſpirit thermometer, and found it nearly as great as Mr. HUTCHINS had ever produced at Hudſon's Bay, that is, about equal to −45° of a ſtandard mercurial thermometer.

PART II.

NO other experiments have been inſtituted, as far as hath come to my knowledge, for the purpoſe of rendering quickſilver ſolid by frigorific mixtures; therefore I now proceed to a new ſeries of facts, which ſerve partly to confirm the former, and partly to ſhew their application. Though the congelation of mercury, abſtractedly conſidered, muſt be allowed to form a very curious and important epocha in the hiſtory of that metal, yet it is as having a reference to thermometers, by teaching us what dependence can be placed upon thoſe inſtruments, fixing our ideas with regard to the different diminutions of heat, and enabling us to form a juſter eſtimate of climates, that it chiefly becomes intereſting to the human race. The ſubſequent part of this narrative will demonſtrate, that quickſilver has very frequently become ſolid by natural cold; that in a few inſtances the effect was ſo palpable and obvious as to ſtrike with immediate conviction; but that in moſt it has never been even ſuſpected till the preſent time, the ſtrange appearances which often occurred being imputed by the obſervers to any other rather than the real cauſe, though they are now found to carry with them a force of internal evidence which eſtabliſhes the truth beyond all doubt.

In enumerating theſe facts, I ſhall continue to purſue a chronological order. They are in general of ſuch a kind as could ſcarcely become an object of attention, till thermometers had acquired ſome degree of accuracy. This did not happen till near the year 1730, and the firſt obſervations which prove the freezing of quickſilver were made within four or five years of that period: ſo intimately are improvements in philoſophy connected with the perfection of inſtruments!

§ 1. When the Empreſs ANNA IWANOVNA had aſcended the throne of Ruſſia, ſhe reſolved to carry into execution one of the favourite ideas of her illuſtrious uncle, PETER the Great, by ſending out proper perſons to explore and deſcribe the different parts of her vaſt dominions, and enquire into the communication between Aſia and America. Three profeſſors of the Imperial Academy were choſen for this expedition; Dr. JOHN GEORGE GMELIN, in the department of Natural Hiſtory and Chemiſtry; M. GERARD FREDERIC MULLER, as general Hiſtoriographer; and M. LOUIS DE L'ISLE DE LA CROYERE, for the department of Aſtronomy; draughtſmen and other proper aſſiſtants were appointed to attend them. In the ſummer of the year 1733 they departed from Peterſburg; and though a principal object of their commiſſion was unavoidably neglected, from the difficulty of tranſporting the neceſſary ſupplies of proviſions to Kamchatka, yet it was the tenth year of their travels before the ſurvivors returned to Europe.

The thermometrical obſervations made in the courſe of this memorable ſurvey of the Ruſſian empire were communicated to the world by Profeſſor GMELIN. His firſt account of them appeared in the preface to his Flora Sibirica[*], where a few of the moſt remarkable are adduced as proofs of the exceſſive rigour of the Siberian climate; but they were afterwards given at full length, with a more ſatisfactory detail of circumſtances, in M. GMELIN's journal of his travels[†], publiſhed by himſelf ſome years after his return. An abſtract of them was alſo inſerted in the Peterſburg Commentaries for the years 1756[‡] and 1765[§], taken, after the profeſſor's death, from his original diſpatches, in poſſeſſion of the Imperial Academy. All the accounts agree together tolerably well; but as the journal is more immediately from the author, in his native language the German, and commonly contains moſt particulars, I thought it right to adhere principally to that work in the following narration of the facts[‖].

It was at Yeniſeiſk, lat. 58°½ N. and long. 92° E. of Greenwich, that M. GMELIN firſt obſerved ſuch a deſcent of his thermometer as, we now know, indicated the mercury to have been frozen. This happened in the winter of 1734 and 1735. "Here," ſays the profeſſor[¶], "we firſt experienced the truth " of what various travellers have related, with reſpect to the " extreme cold of Siberia; for, about the middle of December, " ſuch ſevere weather ſet in, as, we are certain, had never been " known in our time at Peterſburg. The air ſeemed as if " it were frozen, with the appearance of a fog, which did not

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[*] P. lxxi—lxxiii.

[†] Reiſe durch Sibirien.

[‡] Nov. Comment. Petrop. tom. VI. p. 425.

[§] Ibid. tom. XI. p. 320.

[‖] See alſo Mem. de l'Acad. des Sciences, 1749, p. 1.

[¶] Reiſe, Theil. I. p. 355.

" ſuffer the ſmoke to aſcend as it iſſued from the chimnies. " Birds fell down out of the air as if dead, and froze imme- " diately, unleſs they were brought into a warm room. " Whenever the door was opened, a fog ſuddenly formed round it. " During the day, ſhort as it was, parhelia and haloes round the " ſun were frequently ſeen, and in the night mock moons and " haloes about the moon. Finally, our thermometer, not ſub- " ject to the ſame deception as the ſenſes, left us no doubt of " the exceſſive cold; for the quickſilver in it was reduced" [on the 5th of Jan. O. S.] " to – 120° of FAHRENHEIT's ſcale, " lower than it had ever hitherto been obſerved in nature."

Thus far Profeſſor GMELIN. Little did he conceive that, though his thermometer was not ſubject to the ſame deception as the ſenſes, it lay expoſed to another ſource of error which defeated all his concluſions: for as ſoon as the cold became ſufficiently great to produce any congelation of the quickſilver, it ceaſed to be a meaſure of the temperature; inſtead, therefore, of 120° below 0, the cold moſt probably did not exceed the point of mercurial congelation, or – 39°, but by a very few degrees, the great deſcent of the quickſilver, as it depended upon its contraction in the act of freezing, only affording a proof that it had really ſuffered this change.

We muſt here obſerve, that Dr. GMELIN's thermometers were conſtructed by M. JOSEPH NICHOLAS DE L'ISLE, brother to the gentleman who went upon this expedition, on the principle invented by himſelf, and which ſtill bears his name. At preſent ſuch thermometers are always made by determining two fixed points, of which the uppermoſt, or that of boiling water, is aſſumed as 0, and the lowermoſt, or the point of melting ice, as 150°, the ſcale being counted downward; but in their original conſtruction, when the utility of fixed points was leſs underſtood, M. DE L'ISLE took the degrees of his ſcale from decrements in the bulk of the quickſilver equal to ten thouſandth parts of its whole volume at the heat of boiling water. By this method the freezing point ſeems to have fallen about the 152d degree*; and accordingly Profeſſor GMELIN, whenever he has occaſion to expreſs his obſervations in the numbers of FAHRENHEIT's ſcale, reduces them on that ſuppoſition. It is not eaſy to diſcover the exact time when the preſent method of reckoning M. DE L'ISLE's degrees commenced; but ſo early as in M. BRAUN's experiments it is expreſſly ſtated, that the freezing point of his thermometer was only 150°†. There can be no doubt, however, both from theory and from WEITBRECHT's ‡ and GMELIN's obſervations, that in the thermometers uſed during this Siberian journey, the degree at which water congealed was nearly as low as 152°; it is according to this proportion, therefore, that I ſhall compute all Dr. GMELIN's obſervations, adhering to the common rule for ſuch as have been made ſubſequent to his time with DE L'ISLE's thermometer.

The next inſtance of mercurial congelation to be found in GMELIN's journal exhibits a very ſtriking example of the force of prejudice. It happened at Yakutſk, lat. 62° N. and long. 130° E. in the winter of 1736 and 1737, and is thus related by the profeſſor §. "This winter was unuſually mild here, "nevertheleſs we endured at times very ſevere cold, being froſt-"bitten in a ſledge within the ſpace of ſix minutes, notwith-"ſtanding all our precautions. One day, alſo, a certain per-

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* GMELIN's Reiſe. Theil. III. p. 143.

† Nov. Comment. Petrop. tom. XI. p. 299.

‡ Ibid. tom. X. p. 303.

§ Reiſe. Theil. II. p. 451.

"ſon, who has ſome reputation in the learned world on ac-"count of his obſervations in natural philoſophy, informed "me by a note, that the quickſilver in his barometer was fro-"zen. I haſtened immediately to his houſe, to ſee this hitherto "incredible wonder of nature. Not feeling by the way the "ſame effects of cold as I had experienced at other times in "leſs diſtances, I began, before my arrival, to entertain ſuſpi-"cions about the congelation of his quickſilver. In fact, I "ſaw that it did not continue in one column, but was divided "in different places as into little cylinders which appeared "frozen, and in ſome of theſe diviſions between the quick-"ſilver I perceived an appearance like frozen moiſture. It im-"mediately occurred to me, that the mercury might have been "cleaned with vinegar and ſalt, and not ſufficiently dried. The "perſon acknowledged it had been purified in that manner. "This ſame quickſilver, taken out of the barometer and well-"dried, would not freeze again, though expoſed to a much "greater degree of cold, as ſhewn by the thermometer. We "were aſſured by the inhabitants, that the ſevereſt cold of this "winter did not approach what they had ſuffered other years; "and yet the thermometer fell ſeveral times to 72° below 0 of "FAHRENHEIT's ſcale, which would be thought, in Germany "at leaſt, a very intenſe froſt."

The gentleman to whoſe obſervation Dr. GMELIN here ſhews ſo little reſpect, ſeems to have been no other than one of his aſſociates in the commiſſion, M. DE L'ISLE DE LA CROYERE*, probably the firſt perſon upon earth who ſaw quickſilver reduced to a ſolid form by cold, and ventured to credit the teſtimony of his ſenſes. As to the objection, that the ſame mercury did not freeze with a greater degree of cold, it is of no avail; for M.

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[*] See Dr. HIMSEL's Letter, Phil. Tranſact. vol. LI. p. 673.

GMELIN had not any other means of eſtimating this but by the deſcent of his thermometer, which could be depended upon no farther than to the point of mercurial congelation. Nay, it is not improbable, that the more violent the cold, the leſs would the quickſilver appear to ſink below that point, from the quicker freezing and adheſion of the ſmall thread of mercury in the thermometrical tube. Beſides, a part of the quickſilver expoſed to the air might eaſily be frozen, and yet no appearance of ſuch a change be perceived, if the maſs did not any where ſeparate or divide. And the fact, that it actually did freeze ſeveral times during the winter is put beyond all doubt, by the ſinking of the thermometer ſo many degrees below the term at which that effect begins to take place. The abſurd idea, that quickſilver appears to congeal in conſequence of water it contains, was derived, I believe, originally from a whim of RAYMOND LULLY's. It has been the uſual refuge of thoſe gentlemen who thought proper to deny that mercury could be made ſolid by cold; but is too deſtitute of ſupport to merit confutation.

Profeſſor BRAUN mentions, on two different occaſions *, that the Peterſburg Academy have in their poſſeſſion ſome obſervations made in Siberia, which ſeem to ſhew the congelation of mercury by natural cold; but that little credit was given to them, becauſe it had at other times been found to retain its fluidity when the cold was much more intenſe. Probably the obſervations here meant are M. DE L'ISLE DE LA CROYERE's; for that gentleman certainly tranſmitted papers to the Academy, according to which the mercury became ſolid as ſoon as it fell about 200° below 0 of his brother's thermometer †, cor-

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* Nov. Comment. Petrop. tom. VIII. p. 363. and tom. XI. p. 269.

† Phil. Tranſact. vol. LI. p. 673.

reſponding with – 25° of FAHRENHEIT's ſcale. This eſtimation, though now found to be many degrees leſs than the truth, yet approaches it ſo near as to impreſs a very favourable idea both of M. DE L'ISLE's talent for obſervation, and of his ſuperiority to vulgar prejudices. It is to this ſame gentleman that we are indebted for an account of the aſtoniſhing ſeverity of the climate in the north-eaſternmoſt extremities of Aſia. Yakutſk itſelf, lying further in that direction than any other place where M. GMELIN reſided, evidently partakes of the ſame rigorous cold, if a winter in which quickſilver froze ſeveral times was eſteemed unuſually mild by the inhabitants.

Another ſet of obſervations, in the courſe of which the mercury frequently congealed, were made by Profeſſor GMELIN at Kirenga Fort, lat. 57½ N. long. 108 E. in the winter of 1737 and 1738. His thermometer on different days ſtood at – 108°, – 86°, – 100°, – 113°, and ſeveral intermediate degrees. Some extraordinary appearances, which very much perplexed him in theſe obſervations, not only admit of a ready ſolution from Mr. HUTCHINS's determination of the freezing-point of quickſilver, but alſo confirm it with wonderful preciſion.

On the 27th of November (O. S.) after the thermometer had been ſtanding two days at – 46°, the profeſſor found it ſunk at noon to 108°. He adds*: "I had ſcarcely noted down "this obſervation, when ſuſpecting ſome miſtake, I ran back "and examined it again. I ſaw the quickſilver now at 102°, "and it continued riſing ſo faſt, that in the ſpace of half an "hour it had reached to – 19°." The explanation of this phænomenon, which appeared ſo odd to M. GMELIN, is very evident. When the intenſe cold ſet in, the quickſilver froze in his thermometer, and ſtuck in the tube at − 46°, that is, a few degrees below the true point of mercurial congelation. But the weather becoming milder two days afterwards, the ſmall thread of quickſilver in the tube ſoon melted, and conſequently ſubſided. Poſſibly it came much lower than Dr. GMELIN happened to obſerve it at noon; for then the great body of quickſilver was undoubtedly in motion, aſcending rapidly as it expanded by melting, till it came up to the degree that correſponded with the temperature of the air. Therefore, inſtead of a change in that temperature from − 46° to − 108° in a few hours, and from − 108° up to − 19° in half an hour, which would have been really aſtoniſhing, this obſervation only ſhews that the cold, after having continued two days as much below − 39° as was ſufficient to freeze mercury, at length abated 20 or 30 degrees, perhaps very gradually; no greater alteration than frequently takes place in moſt extra-tropical climates.

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* Reiſe. Theil. II. p. 619.

A ſimilar inſtance occurred at Kirenga Fort a few days afterwards *, explicable in the ſame manner.

Again, on the 29th of December (O. S.) Dr. GMELIN found his thermometer, which had been ſtanding at − 40° early in the morning, ſunk down to − 100° at four in the afternoon. He ſubjoins the following remark †. "I obſerved ſome air "in the thermometer, ſeparating the quickſilver for the ſpace "of about ſix degrees. Yeſterday evening I took notice of a "ſimilar appearance, except that the air was not then collected "into one place, but lay ſcattered in ſeveral. I conſidered it "as an accidental fault in the inſtrument, and attempted to "expel it by means of a ſteel wire, but could not bear the "cold. In the barometer, alſo, ſome very ſmall air-bubbles

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* Reiſe. Theil. II. p. 625.

† Ibid. p. 631.

"were

"were perceived. Next morning only a very few minute air-"bubbles remained in the quickſilver of the thermometer, "which had then riſen to − 44°, and not the leaſt veſtige of "them was to be ſeen in the barometer."

It cannot be doubted, but theſe appearances proceeded from a congelation of the mercury in Profeſſor GMELIN's inſtruments. His thermometer ſhewed by its deſcent that the cold was ſufficient for this effect; and the diſappearance of thoſe ſuppoſed air-bubbles as the froſt abated, demonſtrates that they were nothing more than interſtices formed by minute portions of congealed metal reſting irregularly upon one another, and which, therefore, were gradually obliterated as the ſolid bits melting down united into one maſs. Now this obſervation is of conſequence, not only as proving that the quickſilver congealed, but likewiſe as pointing out, with great exactneſs, the degree of cold neceſſary for its congelation. For ſince, when only a few very minute bubbles were left, the mercury reached up to − 44° in the thermometer, its freezing point could not be below that degree, becauſe ſome of it continued ſtill ſolid, but muſt be a little higher, juſt ſo much as would anſwer to the expanſion produced by the melting of that very ſmall proportion of metal which remained frozen.

From Dr. GMELIN's attempt to extricate the ſuppoſed air by means of a wire, it would ſeem, that the tubes of his thermometers were open at top. This idea is in ſome meaſure confirmed by a paſſage in the preface to his Flora Sibirica *, where he mentions, that upon arriving at, or quitting a place, he uſed to try whether his thermometer would riſe to 0 in boiling water, and, if there appeared any deficiency, corrected it by the addition of freſh quickſilver; which he would ſcarcely have

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* P. lxxiv.

done, had the tube been ſealed, on account of the great riſk of ſpoiling the inſtrument in breaking it open ſo often. Perhaps the above-mentioned appearance of air bubbles, from the divided ſtate of the frozen quickſilver, may have depended in part upon this expoſure to the atmoſphere, as well as upon the large ſize of M. DE L'ISLE's original thermometers. Under theſe circumſtances we cannot ſuppoſe that the inſtruments were very exact.

Dr. GMELIN on ſeveral other occaſions obſerved, that the quickſilver in his thermometer looked as if air was interſperſed in it. Whenever this happened, it always ſubſided many degrees below what we now underſtand to be the point of mercurial congelation *. The profeſſor, totally at a loſs to explain ſuch a phænomenon, imputes it ſometimes to a fundamental fault in his inſtrument, but which he could never diſcover, and at other times to an imaginary effect of the intenſe cold, in expelling or extricating air from the pores of the quickſilver, to be abſorbed as the cold abated. On the 9th of January, 1738, O. S. the mercury ſunk at once to – 114°, after having been ſtationary two whole days at – 45° †.

The laſt obſervations of M. GMELIN's, in which quickſilver froze, were made upon his return homeward in a part of Siberia, much nearer the confines of Europe. During the month of December, 1742, as he was paſſing over that branch of the Ural or Riphæan mountains which runs between Verchoturie and Solikamſk, about the 59th degree of N. lat. and ſcarcely 60 degrees E. of Greenwich, his thermometer ſunk to – 41°, – 70°, and at length into the bulb, though it was graduated

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* Reiſe. Theil. II. p. 634.

† Ibid.

to 96° below 0 [*]. The ſame appearance of air bubbles which he had ſo frequently remarked in ſuch great deſcents of the thermometer, puts it beyond doubt that the quickſilver was frozen. This event furniſhed a very ſtriking proof of the force of habit in reconciling men to hardſhips, which in their common courſe of life are thought intolerable. Profeſſor GMELIN, who had now been nine years in Siberia, not only bore to travel in this exceſſive cold, but alſo, in order to aſcertain the height of the mountains he traverſed, employed himſelf in obſerving a barometer, whilſt the quickſilver was freezing in his inſtruments.

Theſe are the principal of Dr. GMELIN's thermometrical obſervations. He collected many more, part of which were deſtroyed by fire or other accidents, and the remainder ſeem to contain no further information. They were conſidered by him as demonſtrating the cold of Siberia to exceed that even of the moſt northern parts of Europe near 100 degrees, an opinion which has ſince been almoſt univerſally adopted; whereas we have, in fact, no proof that the difference of climate amounts to ſo much as the variation between one winter and another. At Yeniſeiſk, where the cold was ſo intenſe in 1735, it does not ſeem to have ever been ſufficient to freeze a thermometer in the winter that M. GMELIN ſpent there four years afterwards; and it will ſoon be ſhewn that quickſilver has congealed more than once in Europe. All that we are authoriſed to conclude, therefore, with reſpect to the Siberian climate, is, that the cold there not unfrequently exceeds the degree indicated by − 39° of a ſtandard mercurial thermometer.

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[*] Reiſe. Theil. IV. p. 512−515.

§ 2. This was the period of ſcientifical enterpriſe. Soon after the intelligent Academicians of Peterſburg had penetrated into Siberia by order of the Ruſſian Monarch, another potent Sovereign ſent out thoſe philoſophical expeditions, by which the opinion of our illuſtrious countryman, reſpecting the figure of the earth, was ſo honourably confirmed. As it became neceſſary, for the determination of this queſtion, to meaſure a degree at the arctic circle, the gentlemen who undertook it were unavoidably expoſed to a great ſeverity of cold. About the time when the quickſilver was exhibited frozen to Profeſſor GMELIN, near the extremity of Aſia, without overcoming his prepoſſeſſion, M. MAUPERTUIS and his aſſociates ſaw the liquor congeal in their ſpirit-thermometer at Torneå in Lapland *. Their mercurial thermometer ſunk at the ſame time to − 37° of M. DE REAUMUR's ſcale; which, if the inſtrument was exactly graduated according to that philoſopher's original idea, would undoubtedly ſhew that the quickſilver froze, as it correſponds with 51° of FAHRENHEIT. But the inaccuracies in conſtructing M. DE REAUMUR's thermometers have been ſo great, that I think no dependence can be placed upon this obſervation, eſpecially as it does not appear to have been attended with any extraordinary phænomenon.

The ſame objection holds good with regard to the obſervations made by M. GAUTIER at Quebec, from the year 1743 to 1749, an extract from which is inſerted in the Memoirs of the French Academy of Sciences †. The account given of his thermometer is too indefinite to allow any certain inference to be

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* OUTHIER Voyage au Nord, p. 145.

† Mem. de l'Ac. des Scienc. 1744, p. 135; 1745, p. 194.; 1746, p. 88.; 1747, p. 466.; 1750, p. 309.

be drawn; but as the quickſilver ſeveral times contracted ſo much as to leave a viſible vacuity in the top of the bulb, and the ſcale ſeems to have reached near to its point of congelation, I am rather of opinion that it actually froze. If ſo, Quebec, ſituated in lat. 47°, is the moſt ſouthern place in which ſuch a great degree of natural cold has hitherto been obſerved.

§ 3. We come now to an inſtance of what, however often it may have happened, has hitherto never been ſuſpected, the congelation of quickſilver in Europe by natural cold. The obſervations which prove this fact are recorded in the Tranſactions of the Royal Academy of Sciences at Stockholm, whence I have extracted the following account, from the original Swediſh.

In January, 1760, the weather was remarkably cold in Lapland. On the fifth of that month different thermometers ſunk to −76°, −128°, or lower *. Again, on the 23d and following days, they fell to −58°, −79°, −92°, and below −238° into the ball †. This great deſcent of the mercury was obſerved in four places, Torneå, Sombio, Iukasierf, and Utsioki, all ſituated between the 65th and 70th degrees of N. lat. and the 21ſt and 28th of eaſtern longitude, by M. ANDREW HELLANT, œconomical Inſpector of Lapland, whoſe remarks on the phænomenon afford of themſelves ſufficient evidence, that the quickſilver was frozen. "During the cold weather at Sombio," ſays he ‡, "as it was clear ſun-ſhine, though ſcarcely "the whole body of the ſun appeared above the low woods that "terminated our horizon, I took a thermometer which was

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* Kongl. Vetenſk. Acad. Handlingar. vol. XX. p. 314.

† Ibid. vol. XXI. p. 312.

‡ P. 314.

" hanging before in the ſhade, and expoſed it to the riſing ſun
" about eleven in the forenoon, to ſee whether, when that luminary was ſo low, it would produce any effect upon the
" inſtrument. But to my great ſurpriſe, upon looking at it
" about noon, I found that the mercury had entirely ſubſided
" into the ball, though it was ſtanding as high as −61° at eleven o'clock, and the ſcale reached down to 238° below 0. I
" could not perceive or think, that the air had changed ſo ſuddenly to ſuch an extraordinary degree of cold. I therefore
" brought my thermometer, the only one I had left, within
" doors, and held it before the fire, when it quickly aſcended
" to the uſual height in a warm room. Upon being carried
" out again into the open air, and placed in the ſhade, it ſunk,
" as in the forenoon, to −61°. Afterwards I expoſed it once
" more to the ſun-ſhine; but the ſun having already begun to
" diſappear behind the horizon, the quickſilver did not ſubſide
" into the bulb as before. I then returned with the inſtrument
" into the room, and held it in my hands before the fire, upon
" which the quickſilver fell back into the ball, where it left a
" vacuum or hollow bubble about the ſize of a pepper-corn.
" When I inclined the thermometer, this bubble ran round the
" ball; and after a few minutes the quickſilver roſe again to
" its former height.

" I repeated theſe experiments ſeveral times at Nebuloslock" [a ſettlement about ten miles diſtant from Sombio] " the ſame afternoon and the following day, by carrying the thermometer
" out of the cold ſometimes to the fire, and ſometimes into a
" warm hut; when the ſame thing happened, that the quickſilver always ſubſided into the ball. After my return to Torneå in April, I attempted to perform them again; but when
" the cold was only a few degrees below the freezing point of

"water, I could never make the mercury ſink, either by holding it before the fire, or carrying it into a warm room, though "the experiments always ſucceeded when the thermometer "previouſly ſtood at – 58° or lower.

"People who were well cloathed, and in briſk motion, or "driving with rein-deer, could bear this cold an hour or two; "but ſuch as travelled with horſes in a ſledge ſoon found it neceſſary to have recourſe to the farm-houſes to warm themſelves. On going out from a hot room, ſome of the firſt "inſpirations were rather heavy and difficult; but the breathing ſoon became eaſier. It felt dreadfully cold; but ſtill I "could not perceive, from the ſenſation alone, that the weather was ſo extraordinarily ſevere as it appeared to be by the "inſtruments. Having ſpent an hour at the houſe of one of "my friends, in the afternoon of the 25th of January, and "finding on my return that a thermometer, graduated to "– 58°, had ſunk into the ball, I could not at firſt believe it "had happened from the cold, but thought the inſtrument "muſt be broken, till other thermometers that were hanging "near it convinced me of the truth."

Several reflexions preſent themſelves on the peruſal of theſe obſervations. The phænomena fairly ſhew, that there was a ſufficient degree of cold to congeal the quickſilver in Mr. HELLANT's thermometers, which ſometimes ſunk regularly into the bulb, but commonly ſtuck faſt in the tube till it was heated by the ſun, the fire, or a warm room, and thus made to ſubſide. The continuance of this cold was very remarkable; it laſted no leſs than three days, with ſufficient intenſity to freeze mercury; a circumſtance almoſt unparalleled any where, and the more extraordinary, becauſe M. HELLANT, during twenty-three years that he had made obſervations in Lapland, never before ſaw the thermometer ſo low as to indicate a congelation of the mercury. But it was not in Lapland alone that the ſeaſon was uncommonly ſevere. At this ſame time the froſt was nearly, if not quite, intenſe enough at Peterſburg to freeze quickſilver, as appears from the remarks of M. BRAUN, who was then engaged in his experiments. And it is a curious coincidence of events, that on the very day when the congelation of mercury by artificial means was firſt clearly eſtabliſhed in Ruſſia, nature ſhould be performing the ſame operation before the eyes of an attentive and philoſophical obſerver in a neighbouring kingdom, who yet had not ſufficient ſagacity to divine her ſecret.

Two circumſtances, however, ſtruck M. HELLANT, which might have led him immediately to ſuſpect the truth. The firſt was, that ſuch a degree of cold as this prodigious deſcent of the thermometer ſeemed to indicate, bore no ſort of proportion to the general ſtile of the weather in that country. What could be more incredible, than that the cold, which had never been known before to ſink the thermometer below $-40°$, ſhould on one particular occaſion exceed that point by hundreds of degrees, more than double the whole variation of temperature between ſummer and winter? Such an event would ſhew a want of balance in the ſyſtem of nature, with reſpect to heat and cold, ſo very different from the apt adjuſtment of its other parts, as to be inadmiſſible but upon the moſt deciſive proofs. Any reflecting perſon, therefore, would be more inclined to believe, that the inſtruments employed had ceaſed to be meaſures of the temperature, from ſome cauſe or other, than that the extremes of cold ſhould be ſubject to ſuch anomalous exceſſes.

The other circumſtance of which M. HELLANT takes notice, that the ſenſation attending this cold by no means correſponded with its effect upon a thermometer, pointed directly to the ſame concluſion. When that inſtrument is at 40° below 0, the beams of houſes crack with a loud exploſion, trees ſplit and are killed, birds fall down dead out of the air, and it is with the utmoſt difficulty that man, notwithſtanding all his reſources, can preſerve the extreme parts of his body from being deſtroyed by the froſt. Now, if the cold had been increaſed as far beyond this degree, as *it* differs from the heat of boiling water, could M. HELLANT have expoſed himſelf to the open air with impunity? The analogy of all we know of cold declares the contrary: and though the power of animals and vegetables to reſiſt variations of temperature has been found much greater than was formerly imagined, I think it would not be raſh to affirm, that in any part of our globe where the cold was carried to ſuch exceſs, the whole ſyſtem of organized bodies muſt periſh.

Yet theſe obvious inferences ſeem to have never occurred to M. HELLANT. Even the unexpected deſcent of his thermometer on being expoſed to heat, ſtrange and inexplicable as it muſt have appeared, and contradictory to all the notions he entertained, did not ſuggeſt to him a doubt of the inſtrument's marking the real temperature of the air. But we now know that it ceaſed to do ſo after the cold had increaſed a few degrees below $-39°$; that all the unuſual phænomena turned upon the congelation of the quickſilver; and that the ſeverity of this ſeaſon, though greater than uſual in Lapland, did not exceed that of common winters by any ſuch remarkable difference.

The vacuum or hollow bubble, obſerved after the quickſilver had fallen back into the ball, ſhews how very much it had contracted by the congelation. This bubble moved upon inclining the thermometer, becauſe the external part of the frozen maſs in the ball having melted before the internal, though not in ſufficient quantity to fill its whole capacity, ran round it freely in a fluid ſtate, the empty ſpot always riſing to the top.

§ 4. Early in the ſpring of 1761, the Abbé CHAPPE D'AUTEROCHE, in his journey to Tobolſk for obſerving the tranſit of Venus, paſſed through Solikamſk, a town of Siberia, ſituated in 59°½ N. lat. and 57° E. of Greenwich. On this occaſion he takes notice*, that the thermometer had ſunk there the preceding winter to − 124°; which, if the general ſtile of the Abbé's remarks will allow ſufficient dependence to be placed upon it, would neceſſarily ſhew that the quickſilver was then frozen.

§ 5. M. ERICH LAXMANN, late Profeſſor of Mineralogy and Chemiſtry at Peterſburg, was reſident in 1765 at Barnaul in Siberia, lat. 53° N. and long. 81° E. as miniſter to the German congregation of the Kolyvan Province. On the firſt day of that year, he ſaw the thermometer down ſo low as − 58° †; whence it is probable, that ſome part at leaſt of the quickſilver was congealed. As no concomitant circumſtances are recorded with this fact, it would ſcarcely have been worth mentioning, were it not to introduce an account of ſome inſtruments, which became afterwards the ſubject of very curious obſervations. For M. LAXMANN, during his abode in this remote country, employed his leiſure in the conſtruction of barometers and thermometers, an art in which he acquired great ſkill. Theſe he afterwards diſtributed, free of expence, to all parts of Siberia where they were likely to be uſed, with the moſt laudable and exemplary zeal to diffuſe ſome rays of ſcience through thoſe dark and uncultivated regions[*].

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* Voyage en Siberie, p. 84. and 93.

† LAXMANN's Sibiriſche Briefe, p. 97.

All the particulars here mentioned are extracted from M. LAXMANN's *Siberian Letters*; a ſcarce book, becauſe, on his return to Europe, he bought up every copy he could find, as they had been publiſhed without his conſent by Profeſſor SCHLÖZER of Gottingen.

§ 6. The benefits accruing from the travels of learned men, could not eſcape the penetration of the wiſe Empreſs who now reigns in Peterſburg. Soon after her eſtabliſhment on the throne, ſhe ordered an expedition of the ſame nature as that in which Profeſſor GMELIN had been engaged above thirty years before. Among the gentlemen who undertook this ſecond philoſophical ſurvey of the Ruſſian Empire, was Dr. PETER SIMON PALLAS, one of the moſt eminent naturaliſts and ſkilful obſervers of the preſent age. The journal of his travels is publiſhed by himſelf in the German language, and comprehends a rich ſtore of curious and uſeful information. In general his winters were not ſpent in the coldeſt parts of Aſia; twice, however, he reſided at Kraſnoyarſk lat. 56°½ N. long. 93° E. and the laſt time, in 1772, had an opportunity of witneſſing the moſt remarkable inſtance of the congelation of mercury by natural cold that is yet known to the world.

" The winter," ſays M. PALLAS[†], " ſet in early this year, " and was felt in December with uncommon ſeverity. On the " 6th and 7th of that month happened the greateſt cold I have " ever experienced in Siberia; the air was calm at the time,

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[*] Sibiriſche Briefe, p. 29.

[†] Reiſe durch verſchiedene Provinzen des Ruſſiſchen Reichs, Theil. III. p. 417.

“ and ſeemingly thickened, ſo that, though the ſky was in other reſpects clear, the ſun appeared as through a fog. I had only one ſmall thermometer left, on which the ſcale went no lower than −70°; and on the 6th in the morning I remarked that the quickſilver in it ſunk into the ball, except ſome ſmall columns which became ſolid and ſtuck faſt in the tube. By the temperature of a room not much warmed, into which I brought the thermometer from the gallery of my houſe, theſe congealed columns immediately fell down; but it was more than half a minute before the mercury came into motion out of the ball. I repeated this experiment frequently, and always with ſimilar ſucceſs, ſometimes one and ſometimes more threads of frozen quickſilver remaining behind in the tube. When the ball of the thermometer, as it hung in the open air, was warmed by being touched with the fingers, the quickſilver roſe; and it could plainly be ſeen, that the ſolid frozen columns ſtuck and reſiſted a good while, and were at length puſhed up with a ſort of violence. In the mean time I placed upon the gallery on the north ſide of my houſe about a quarter of a pound of clean and dry quickſilver in an open bowl; within an hour I found the edges and ſurface of it frozen ſolid, and ſome minutes afterwards the whole was condenſed, by the natural cold, into a ſoft maſs very much like tin. While the inner part was ſtill fluid, the frozen ſurface exhibited a great variety of *branched* wrinkles; but in general it remained pretty ſmooth in freezing, as did alſo a larger quantity of quickſilver which I afterwards expoſed to the cold. The congealed mercury was more flexible than lead; but upon being bent ſhort it was found more brittle than tin, and when hammered out thin it ſeemed ſomewhat granulated. If the hammer had not been per-

" fectly cooled, the quickſilver melted away under it in drops; " and the ſame thing happened when the metal was touched " with the finger, by which alſo the finger was immediately " benumbed. In our warm room it thawed on its ſurface gra" dually, by drops, like wax on the fire, and did not melt all " at once. When the frozen maſs was broken to pieces in the " cold, the fragments adhered to one another, and to the bowl " in which they lay. Although the froſt ſeemed to abate a " little toward night, yet the congealed quickſilver remained " unaltered, and the experiment with the thermometer could " ſtill be repeated. On the 7th of December I had an oppor" tunity of making the ſame obſervations all day; but ſome " hours after ſun-ſet a north-weſt wind ſprung up, which raiſed " the thermometer to −46°, when the maſs of quickſilver " began to melt."

Before this obſervation of Dr. PALLAS's, no perſon had ſeen or handled quickſilver frozen by natural cold, ſo as to ſubmit the fact to the public with competent evidence; but the circumſtances here related are ſo pointed and conſiſtent, that even thoſe who had doubted of M. BRAUN's experiments were now ſtaggered, and began to believe. Indeed, it was ſcarcely poſſible to ſuppoſe any miſtake, when Dr. PALLAS had two whole days to repeat and vary the experiments at his leiſure. But beſides removing all doubts upon the congelation of quickſilver, theſe obſervations tended to ſhew, within certain limits, the degree of cold neceſſary for that effect. It was evident that the freezing point muſt be ſomewhere above 70°, becauſe the thermometer's graduation reached only ſo low, and yet ſome part of the mercury always congealed in the tube; and as the ſolid maſſes did not begin to melt till the thermometer roſe to −46°, *that* ſeemed to be nearly the point at which it paſſes from a ſolid to a fluid ſtate, and very poſſibly was ſo upon this inſtrument, a difference of ſeveral degrees being often found in thermometers ſo low down on the ſcale as −40°, from inaccuracies in their conſtruction.

The cryſtallization of quickſilver, alſo, became manifeſt on this occaſion. Hence, when hammered out thin, it ſhewed a granulated texture. The branched wrinkles too, which formed on its ſurface whilſt it was congealing, could ſcarcely have proceeded from any other cauſe, and ſuggeſt a general idea of the manner in which it ſhoots. That quickſilver ſhould cryſtallize ſo much more viſibly than moſt other metals, will not appear ſurpriſing, if we conſider how little the cold is below its freezing point. Such ſubſtances as require, in order to melt, a degree of heat much above that of our atmoſphere, experience ſo great a change of temperature upon being taken off the fire, that they become ſolid haſtily, and as it were in confuſion; whereas quickſilver, having never probably been expoſed to a degree of cold much exceeding that of its melting point, its particles have had full leiſure to arrange themſelves regularly, in exact conformity to the laws of their mutual attractions. As in Profeſſor BLUMENBACH's and Mr. HUTCHINS's experiments, ſo here, I imagine, ſome ſlight roughneſs of the ſurface was occaſioned by this cryſtallization; in conſequence of which M. PALLAS compared his frozen quickſilver to tin, rather than to bright ſilver, the appearance it always aſſumes when congealed in ſmooth glaſs.

Another property of quickſilver, very important to be known, was obſerved perhaps no where ſo diſtinctly as on this occaſion at Kraſnoyarſk; I mean its tendency to adheſion in freezing. Thus, Dr. PALLAS ſays the fragments of the congealed maſs ſtuck to one another, and to the bowl in which they lay. So likewiſe Mr. HUTCHINS found the frozen quickſilver adhering to his cylinders and gallipot; Profeſſor BLUMENBACH to his glaſs veſſel; and ſimilar facts occurred to other obſervers. Hence the deceptions, already ſo often mentioned, from the ſticking of the mercury in the ſtems of thermometers. And this cauſe of error can ſcarcely ever fail to take place; for if quickſilver congealing in wide open veſſels adheres to them wherever it touches, how can it be expected to remain looſe when frozen in a narrow tube? Now, ſince quickſilver, under theſe circumſtances, retains the ſame appearance as while fluid, from the poliſh given to its ſurface by the ſmooth glaſs, it is no wonder that ſuch frequent miſtakes have been made relative to the height of the thermometer, both in experiments with artificial cold, and in meteorological obſervations. At the ſame time it muſt be confeſſed, that ſuch a tendency to adhere, in a metal which contracts ſo much in becoming ſolid, is not a little difficult to explain, unleſs we may ſuppoſe it to be the immediate effect of the cryſtallization.

Quickſilver, with all its other qualities of a perfect metal, ſeems from Dr. PALLAS's, and indeed moſt of the experiments, not to be completely malleable, but rather apt to break under the hammer. Perhaps it has never been ſufficiently cooled to poſſeſs its metallic properties in perfection; for with reſpect to its melting point it may be conſidered as having always been hot, that is, heated near to fuſion, a ſtate in which other metals undergo a very ſenſible change in their properties. But when mercury congeals in veſſels which confine its ſurface, it ſeems to become more malleable than under a looſe cryſtallization.

Dr. PALLAS's travels laſted from the year 1768 to 1773, during which time *this* of the beginning of December, 1772, ſeem ſeems to be the only inſtance that occurred of the freezing of quickſilver. From this circumſtance, and his occaſional hints about the climate, I am inclined to ſuſpect, that the cold was not ſo great in Siberia at this period, as when Dr. GMELIN was there from 1734 to 1742. In Europe, likewiſe, the winters in general appear to have been ſeverer about the time of GMELIN's expedition than lately. It was in this very town of Kraſnoyarſk that Profeſſor GMELIN reſided during the famous hard winter of 1739 and 1740; but unfortunately he has informed us of the cold there only by its groſſer effects, I believe becauſe his thermometers had been accidentally deſtroyed.

§ 7. Nearly 500 miles ſouth-eaſtward of Kraſnoyarſk is the town of Irkutſk, the capital of a Siberian Province on the vaſt Baikal Lake, and ſituated in lat. 52° N. and about the 104th degree of E. longitude. At the former of theſe places, the cold recorded by Dr. PALLAS began to abate on the 7th of December in the evening; and more than a day after, that is, on the 9th in the morning, it became ſo intenſe at Irkutſk as to freeze quickſilver. An account of this phænomenon, ſent by Lieutenant-general VON BRILL, the governor, was publiſhed by Dr. PALLAS in his Journal[*], and M. GEORGI, one of the aſſociates in this expedition, afterwards collected ſome further particulars[†]. It appears, that about four in the morning, the quickſilver was found frozen in the barometer and thermometer, its upper ſurface being irregularly broken. In the former of theſe inſtruments the mercury ſtood at 28 inches 7 lines, and the broken appearance extended through a ſpace of about

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[*] Ibid. p. 419.

[†] Bemerkungen einer Reiſe im Ruſſiſchen Reich, B. I. p. 29.

5 lines from the top downward: when it came to melt a few hours afterwards, it roſe to 29 inches 7 lines, which difference of height was probably in part at leaſt an effect of the contraction it undergoes in freezing, its greater ſpecific gravity in the congealed ſtate making it ſtand proportionably lower. In the thermometer, part of the mercury had ſtuck at −44°; and immediately under −59° an empty ſpace was left, equal to 11 degrees of the ſcale. This obſervation, therefore, determines almoſt preciſely the freezing point; for none of the quickſilver could have adhered in the tube ſo high as −44°, unleſs it had congealed before it ſunk below this point, and conſequently before the cold exceeded this degree. And that the mercury was really frozen became evident afterwards; for about eleven in the forenoon, as the air grew warmer, it was found to have all ſubſided into the bulb, the ſmall threads in the tube melting down into the vacuity left there, out of which it did not riſe again till near two hours had elapſed.

The accuracy of this thermometer at Irkutſk, apparent from its preciſion with reſpect to the point of congelation, would be matter of ſurpriſe, had we not been informed, that both it and the barometer were among the inſtruments conſtructed by M. LAXMANN during his reſidence in Siberia. M. GEORGI mentions them as being in the poſſeſſion of Dr. WACHSMANN, the public phyſician of Irkutſk, by whom probably the obſervations were communicated to the governor.

§ 8. As the cold of America is well known to exceed that which prevails under the ſame latitudes in Europe, we muſt expect to find quickſilver freezing ſpontaneouſly in parts of that continent which do not lie very far to the northward. Accordingly, beſides the inſtance of Quebec formerly mentioned, this effect takes place frequently in Hudſon's Bay, even at Albany Fort, where the latitude is not one degree greater than in London. Mr. HUTCHINS, in his different ſituations at Hudſon's Bay, has been conſtantly attentive to meteorological obſervations. During his former reſidence at York Fort, ſituated near the middle of the Bay in lat. 58 N. he was not provided with any thermometer graduated more than 70 or 90 degrees below the cypher: but he remarked, that "the quickſilver "frequently ſunk into the bulb," eſpecially after having been ſtationary at − 55° or − 57°, and that it afterwards uſed to aſcend to "about − 30°, indicating a greater degree of "heat than before it fell[*]." Theſe phænomena were clearly owing to its congelation, adheſion in the tube, and ſubſequent liquefaction as the air grew warmer. When Mr. HUTCHINS went afterwards to Albany Fort, and had procured inſtruments with more extenſive ſcales, he obſerved the ſame appearances ſtill more diſtinctly. His thermometers froze twice in the winter of 1774 and 1775, and three times in that of 1777 and 1778; and in every inſtance, except one, the mercury ſunk hundreds of degrees juſt as the cold began to abate[†]. The laſt of theſe obſervations is rendered remarkable by the deſcent of the quickſilver to − 490°, the greateſt ever known by natural cold, and probably very near its extreme term of contraction by freezing. In 1782, alſo, Mr. HUTCHINS's thermometers, together with ſome quickſilver in a phial, again congealed in the open air, and exhibited ſimilar phænomena, as appears from the account of his experiments.

Fortunately in theſe inſtances of intenſe cold at Albany Fort, attention was paid not only to the mercurial thermometers, but likewiſe to one made of ſpirits, whoſe relative movement has been aſcertained by compariſon. This inſtrument, while the others were three, four, or almoſt five hundred degrees below 0, never ſunk further than to a point which correſponds with −46° of a ſtandard mercurial thermometer. Hence it would have been eaſy to infer, both that the quickſilver actually congealed on theſe occaſions, and that the degree of cold neceſſary for ſuch an effect does not exceed −46°. That the moſt intenſe cold of Hudſon's Bay, during a ſeries of ſeveral years, went ſo little below the point of mercurial congelation, well deſerves to be noticed; and as it ſeems to be ſeldom greater in Siberia, at leaſt in the parts viſited by GMELIN or PALLAS, the effects being not more violent, perhaps we are authoriſed to conclude, that the extreme of artificial cold, produced by ſnow and nitrous acid, correſponds pretty exactly with the extreme of natural cold in the moſt rigorous climates which can well be inhabited.

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[*] MS. Journal.

[†] Ibid.

Mr. HUTCHINS's meteorological journal at Hudſon's Bay confirms what he mentions in the remarks on his third experiment, that in the coldeſt weather the wind is to the ſouthward of W. which muſt evidently depend on ſome local circumſtance of that country. During my own reſidence in America, I thought I could diſtinctly perceive that the coldeſt winds, vulgarly called *north-weſters*, did not blow exactly from that quarter; but at Rhode Iſland, for inſtance, came from N.W. by W. or a point ſtill more to the weſtward; at New York were rather nearer the W. than the N.; and at Philadelphia almoſt due N.W. On tracing lines in theſe directions from Albany Fort, Rhode Iſland, and Philadelphia, they are found to meet among the great ridges of mountains which ſeparate Hudſon's Bay from Lake Superior; whence a ſuſpicion ariſes, that theſe mountains may be one ſource of the exceſſive cold which particular winds occaſion in North America. But another circumſtance is to be taken into conſideration, that when the fury of theſe N.W. winds abates, they very regularly draw round to the weſtward and ſouthward. Now the *maximum* of cold muſt be, when the cold wind has blown as long as it can, and the ſucceeding warmer wind has not yet had time to undo any of its effect; conſequently at the period when it is veering round from the northward toward the ſun. Accordingly we find, that when the cold has become ſufficiently great at Albany Fort to congeal quickſilver, the wind not only came from the ſouthward of W. but commonly, alſo, blew with very little force; indeed the intenſeſt froſts in all countries ſeem to take place with light airs or a calm.

§ 9. There are, in the poſſeſſion of the Royal Society, ſeveral other meteorological regiſters from our different ſettlements on Hudſon's Bay; but none of thoſe which I have ſeen contain any ſtriking extraordinary appearances, to ſhew that the thermometer was frozen; and the deſcent alone, within certain bounds, determines nothing, for theſe inſtruments were formerly conſtructed with ſo little accuracy, as to be often marked 8 or 10 degrees too low at the point of mercurial congelation. I conclude, therefore, that when Meſſ. WALES and DYMOND were at Prince of Wales's Fort in the winter of 1768 and 1769, the quickſilver always retained its fluidity, though it once ſunk to – 45° of their thermometer*. This Fort lies in lat. 59°; but on account of the abovementioned ridges of mountains, and perhaps for other cauſes, the ſouthern parts of the Bay may be quite as cold as the northern.

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* Phil. Tranſact. vol. LX. p. 153.

§ 10.

§ 10. We muſt now return to Europe. In the beginning of the year 1780, M. VON ELTERLEIN, of Vytegra, froze quickſilver by natural cold, and ſent an account of his experiment in a letter to the late Profeſſor GÜLDENSTÄDT, then at Peterſburg. I obtained from Gottingen a copy of this letter, in the original German, by the friendſhip of Sir JOSEPH BANKS; and, tranſlated, it is as follows:

“ On the 4th of January, 1780, the cold having increaſed to – 34° that evening at Vytegra, I expoſed to the open air three ounces of very pure quickſilver, in a China tea-cup, covered with paper pierced full of holes. Next day, at eight in the morning, I found it ſolid, and looking like a piece of caſt lead, with a conſiderable depreſſion in the middle. On attempting to looſen it in the cup, my knife raiſed ſhavings from it as if it had been lead, which remained ſticking up; and at length the whole ſeparated from the bottom of the cup in one maſs. I then took it in my hand to try if it would bend; it was like ſtiff glue, and broke into two pieces; but my fingers immediately loſt all feeling, and could ſcarcely be reſtored in an hour and an half by rubbing with ſnow. At eight o'clock a thermometer, made by M. LAXMANN of the Academy, ſtood at – 57°; by half after nine it was riſen to – 40°; and then the two pieces of mercury, which lay in the cup, had loſt ſo much of their hardneſs that they could no longer be broken or cut into ſhavings, but reſembled a thick amalgam, which, though it became fluid when preſſed by the fingers, immediately afterwards reſumed the conſiſtence of pap. With the thermometer at – 39°, the quickſilver became fluid. The cold was never leſs on the 5th than – 28°, and by nine in the evening it had

" increaſed again to −33°. In the morning the wind was " N.N.E. and afterwards N.W."

This experiment of M. VON ELTERLEIN's deſerves attention in many reſpects. It aſcertains the freezing point of mercury with ſuch wonderful exactneſs, from the melting of the ſolid pieces when the thermometer came up to −39°, as to furniſh a valuable corroboration of Mr. HUTCHINS's experiments, and at the ſame time very much to enhance our opinion of M. LAXMANN's ſkill in the conſtruction of inſtruments. When the thermometer was thought, early in the morning, to be ſtanding at −57°, I imagine, that part of the quickſilver being frozen adhered in the tube. The ductility of the ſolid metal muſt have been conſiderable, from its yielding to the knife in the form of ſhavings; yet, as in moſt other inſtances, it ſhewed ſome degree of brittleneſs when force was applied to it in the maſs. Cryſtallizing without impediment, it aſſumed an appearance which M. VON ELTERLEIN rather compares to that of lead than of ſilver. Its tendency to adheſion became evident from the neceſſity of employing an inſtrument to ſeparate it from the tea-cup; and its contraction in freezing was demonſtrated by the depreſſion obſerved in the middle of the ſolid maſs. This ſingle experiment, therefore, exemplifies, in a very beautiful manner, moſt of the properties hitherto diſcovered in quickſilver, when it paſſes from a fluid to a ſolid form.

Vytegra, or Witegorſk, is ſituated in lat. 61° N. and long. 36° E. upon a river of the ſame name. It has acquired ſome celebrity from one of the many uſeful projects which occupied the active mind of Czar PETER the Great. He propoſed to cut a canal from the river Vytegra which diſcharges itſelf into the Lake Onega, to the river Kovſha which joins the Beloſero, or White Lake, in order to form a communication between thoſe two great bodies of water; but the undertaking was unfortunately interrupted by his death *.

§ 11. The laſt inſtance I have been able to find of the congelation of quickſilver by natural cold, occurred no longer ago than the beginning of the year 1782, in Iemtland, one of the northern provinces of Sweden. M. JOHN TÖRNSTEN, Engineer-extraordinary, is the gentleman to whom we are indebted for this obſervation. His letter on the ſubject, dated from Brunflo in Iemtland, lat. 63°½ N. and long. 15° E. is inſerted in the Swediſh Tranſactions for 1782 †, together with ſome remarks upon it by Profeſſor WILCKE.

"During twelve years," ſays M. TÖRNSTEN, "that I have "reſided here in Iemtland, the cold had never but once brought "the thermometer ſo low as – 36°, till the laſt day of December, 1781, when it fell in the evening to – 54°. The following new-year's day it was ſunk to – 56° at eight in the "morning, and by ten to – 62°. Here it continued ſtationary "ſeveral hours, but at half paſt four in the afternoon it was "obſerved at – 116°, and by eight the ſame evening it had "riſen to – 31°. Although the quickſilver," continues M. TÖRNSTEN, "thus fell to – 116° on the firſt of January in "the afternoon, I am of opinion that its deſcent ought not to "be aſcribed to a proportionable increaſe of cold, but on the "contrary proceeded from the ſudden change to milder weather, which came on that afternoon. For the preceding "evening, when the thermometer was ſtanding at – 54°, I "remarked, that, upon bringing it into a warm room, the "quickſilver fell on a ſudden entirely into the ball, which was

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* Büſching's Erdbeſchreibung, Theil I. p. 669.

† Kongl. Vetenſk. Acad. Nya Handlingar, tom. III. p. 80.

" about 130 degrees below 0. This experiment I repeated ſe-
" veral times with ſucceſs, but obſerved the following difference, that if I had not kept the thermometer in the heat long enough for the quickſilver to begin to riſe again after it had ſunk into the ball, it never aſcended above the 130th degree by continuing in the cold, but upon being carried back into the warm room it contracted ſtill more in the ball by a quantity which, however viſible, could not be meaſured. On the other hand, if the inſtrument had been kept in the room till the mercury had riſen above −54°, it became ſtationary at that degree in the open air. Now, though I did not, on the 1ſt of January, bring the thermometer within doors before it had ſunk of itſelf to −116°, yet *this* fall likewiſe ſeems to have been occaſioned by the change to milder weather which was then taking place. For at eight in the evening, when the external cold was at −31°, I found that hoarfroſt formed on the ball and ſtem of the thermometer as before, upon its being brought into a warm room; but the mercury did not ſink, on the contrary it began immediately to riſe.

" Some quadrupeds periſhed by the intenſe cold, and a great number of ſmall birds were found dead. Nevertheleſs, the people did not neglect going to church on this high holiday, and I have not heard that any one was froſt-bitten who went out with proper cloathing."

M. TÖRNSTEN certainly judged right when he concluded, that the fall of the thermometer to −116° rather indicated a diminution than an increaſe of the cold. Though he knew nothing of the cauſe, yet his obſervation led him to a juſt inference, in which he diſplayed more ſagacity than M. HELLANT on a ſimilar occaſion. All the phænomena which ſo much perplexed theſe gentlemen are explicable in the following manner. When the air becomes ſufficiently cold to freeze quickſilver, that metal muſt be ſtanding about − 39°, or, in the common way of marking the boiling point, ſomewhere between − 40° and − 50°, in the tube of a thermometer expoſed to it. As the ſmall thread of mercury in the tube muſt be more eaſily affected by the cold, it will probably congeal before any other part, and ſtick faſt about the abovementioned degrees. The remainder of the mercury will then go on to freeze, and as it ſuffers ſuch a great contraction in becoming ſolid, muſt leave a conſiderable vacuity in the bulb of any common thermometer. Conſequently, when the cold, from whatever cauſe, comes to be leſs than is required for keeping the metal in a ſolid ſtate, the ſmall thread that was frozen in the tube immediately melts, and ſinks down into the vacuity of the bulb, where the whole maſs remains, till by its gradual liquefaction it expands again into the tube, and becomes a juſt meaſure of the temperature. This agrees exactly with what M. TÖRNSTEN obſerved. In the evening of the 31ſt the quickſilver congealed in his thermometer, and part of it ſtuck in the tube at − 54°, but ſubſided into the vacuity left in the bulb, as ſoon as it was expoſed to heat. When the inſtrument had been kept in the warm room till the quickſilver re-aſcended into the tube, it froze and adhered again in the open air, and the ſame phænomena were repeated. If M. TÖRNSTEN be exact in ſaying it always became faſt at − 54°, the circumſtance is curious, and may have depended upon ſome particular ſtate of the tube in that part, or upon the firſt ſhooting of the mercury after it had been cooled to a certain degree below its freezing-point. But when the thermometer was carried back into the open air before any of the quickſilver had riſen out of the bulb, the effect of the cold could not be to force it up into the tube, and therefore no ſuch appearances were obſerved as in the former caſe. With regard to M. TÖRNSTEN's remark, that when the whole maſs of quickſilver remained in the ball it ſtill contracted upon the application of heat, the fact is ſo improbable, and would be perceived with ſuch difficulty, that I have no doubt but he was miſled by ſome prepoſſeſſion. In like manner on the 1ſt of January, when the thermometer, having been ſtationary ſome hours at – 62°, ſunk in the afternoon to – 116°, it happened unqueſtionably from the melting and ſubſiding of a thread of frozen mercury, which had adhered in the tube of the inſtrument as high as the former degree. None of theſe effects could be produced when the thermometer had riſen to – 31°, becauſe the cold was not then ſufficient to congeal the quickſilver. In this eaſy and ſimple manner, does our knowledge of the freezing point of mercury enable us to account for phænomena, which were thought ſo anomalous as to elude every kind of explanation. Even ſo lately as laſt year, one of the moſt eminent philoſophers in Europe, Profeſſor WILCKE of Stockholm, made a vain attempt to ſolve the difficulties by a ſtrained application of his doctrine relative to the various ſpecific quantities of heat in bodies, and their different attractions for the matter of heat *.

It would now be ſuperfluous to add, that the real cold at Brunflo was by no means what the thermometer ſeemed to indicate, but probably very little exceeded – 39°, or the degree of mercurial congelation, had not M. TÖRNSTEN's obſervations been lately repreſented, even in this country, as exhibiting an inſtance of cold actually carried to ſuch a diſproportionate and enormous exceſs.

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* Ibid.

Thus is the hiſtory of the congelation of quickſilver, both by natural and artificial cold, brought down to the preſent period. All the facts I have collected are here delivered: it is not improbable, however, that there may be others which have eſcaped me, eſpecially ſuch as are very recent, or have never been publiſhed*; but the number already found is greater than

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* Accordingly, having gone to Paris after this paper was read, and there mentioned, at a meeting of the Academy of Sciences, our late experiments on the congelation of quickſilver, I was informed, that M. CAZALET had ſucceeded in rendering it ſolid at Bourdeaux; and ſoon afterwards the following account of his experiment came out in the Paris Journal, which, for obvious reaſons, I ſhall give in the original French.

“ M. CAVENDISH, de la Société Royale de Londres, a fait dans le mois de Février dernier, l'experience de la congélation du mercure, à Hampſtead, ſitué à deux milles de Londres. Ce Savant eſt ſur le point de publier ſon mémoire à ce ſujet. Le travail du chymiſte Anglois ayant été annoncé à l'Academie Royale des Sciences, dans une de ſes ſéances, ſon Directeur, M. CADET DE GASSICOURT, a *revendiqué* en faveur de M. CAZALET, la congélation du mercure dans un climat beaucoup plus temperé que Londres, le chymiſte François l'ayant obtenue à Bordeaux. . . . .

“ M. CAZALET, regardant l'acide nitreux concentré comme, de tous les ſels, celui qui produit la diſſolution de la glace avec plus de facilité, annonça en 1779, dans une de ſes leçons publiques à Bordeaux, qu'il croyoit à la poſſibilité de la congélation du mercure, par ce moyen, dans les Provinces méridionales mêmes; mais il n'eut occaſion de faire ſon expérience qu'au mois de Septembre de l'année dernière.

“ Il prit de la glace pilée, paſſée à travers un crible, la mit dans un baril dont le fond étoit un plat de porcelaine percé de pluſieurs trous pour faciliter l'ecoulement de la glace fondue; il plaça pluſieurs tubes remplis de mercure au centre de la glace, qu'on arroſa d'eſprit de nitre fumant fait par le procédé de WOULFE. On rapprochoit la glace des tubes à meſure que la diſſolution s'en opéroit; il fallut 120 livres de glace pour produire la congélation du mercure. Les tubes retirés et caſſés les uns après les autres, le mercure ſe trouva en filets comme

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than I expected on beginning the ſearch. By ſuch a connected view of the different obſervations and experiments in any one branch of ſcience, we are furniſhed with the beſt opportunity of diſcriminating what is certain from what is doubtful, and acquire as diſtinct ideas as the actual ſtate of knowledge will admit. On the preſent ſubject of mercurial congelation, the concluſions have in general been noticed, as the premiſes occurred. Though Mr. HUTCHINS's experiments did not ſtand in need of any confirmation, yet ſtill it is pleaſant to ſee their principal reſult, the freezing point of quickſilver, eſtabliſhed by ſuch a body of collateral evidence as, taken together, is abſolutely irreſiſtible. But beſides the information obtained relative to quickſilver itſelf, we have been able to correct ſeveral vulgar prejudices. The difference between cold climates no longer appears ſo prodigious, nor the reſiſting powers of animals and vegetables ſo aſtoniſhing and inconceivable. That extenſive ſcale of heat, which repreſents its diminutions by artificial means as continued down ſo many hundreds of degrees below the greateſt produced by nature, however ſpecious in proſpect, proves to be deſtitute of foundation. The uſe of quickſilver for thermometers is at length fully aſcertained. From the boiling point, to 39° or 40° below 0, it muſt be conſidered as unexceptionable, all ſuſpicion of its irregular contraction within thoſe

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“de l'argent. Bientôt la chaleur de l'atmoſphére lui rendit ſa fluidité. *Cette experience intereſſante a le double merite d'avoir été devinée par un chymiſte François, et exécutée par un autre; il a ſuppléé à ce que le climat oppoſoit d'obſtacles, un moyen tout-à-fait ingenieux, et qui exigeoit des connoiſſances en phyſique.*” See Journal de Paris, 15 Juill. 1783, p. 814.

The peculiarity of M. CAZALET's proceſs conſiſts in the largeneſs of the quantities on which he operated, and the proviſion he made for the uſeleſs liquor, produced by the melted ice, to run off as faſt as it was formed. He certainly congealed the mercury, but did nothing to aſcertain its freezing point, which he ſeems not even to have had in contemplation.

bounds being removed, by ſuch a complete explanation of the cauſe upon which its anomalous deſcent in the lower part of the ſcale depends. On this principle there might, perhaps, be ſome propriety, in conſtructing thermometers of mercury, to fix the cypher at its point of congelation, and thence reckon the degrees of heat upwards.

The principal advantage, however, of thus paſſing in review all former accounts, is to furniſh an important leſſon to authors, which can never be too ſtrongly inculcated, that their accuracy muſt be brought to the teſt of future diſcoveries. As knowledge advances, their errors, their miſrepreſentations, their ſuppreſſion of the truth, or fictitious additions to it, ſhall all be infallibly detected, and heap upon their head proportionable ignominy; while the ſimple and candid narrative, the exact and unbiaſſed relation of facts, will acquire redoubled luſtre from the fiery trial. Let every author recollect, that the day is impending, when ſome unforeſeen improvement, affording means to ſift falſehood from truth, however artfully blended, ſhall finally decide whether he is to be reprobated with the baſe herd of deceivers, or ranked among thoſe faithful votaries of ſcience, whoſe names will be delivered down with honour to poſterity.